Informal Support and Insurance

An Empirical Analysis of the Interplay between
Inter-Household Support Arrangements and
Access to Alternative Risk Management Resources

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Chapter 1

Introduction

1.1 Motivation

When people have limited resources on hand, the immediate social network presents an important source of support. In particular for economically less developed countries this has been well-documented. A large proportion of the population in these countries is self-employed and thus faces highly fluctuating income (Banerjee and Duflo 2007; Charmes 2012). While with the promotion of microfinance, access to credit is nowadays less constrained, financial institutions seldom offer emergency loans and the available credit products are often not suited to deal with day-to-day income fluctuations (Collins et al. 2009; Karlan and Mullainathan 2013). When income is insufficient, people thus borrow informally from neighbors, relatives and friends in order to cover the costs of their daily expenses (Udry 1994; Fafchamps 2008; Kinnan and Townsend 2012); likewise, in rural communities subsistence farmers borrow rice and other crops from neighboring farmers to cope with yield fluctuations and staggered crop cycles (Platteau 2000). In case of health emergencies, the social network assumes insurance functions. In countries where health insurance is non-existent or inaccessible for the poor and state-provided assistance is insufficient, monetary and in-kind support by relatives and friends are shown to be a major strategy to cover health related expenditures and to cope with foregone income (Fafchamps and Lund 2003; De Weerdt and Dercon 2006; De Weerdt and Fafchamps 2011). Support by the social network is not confined to monetary and in-kind transfers. In particular in rural areas, labor markets are often not well developed, and when people are in need of short-term labor assistance, e.g. for the harvest season, they tend to turn to their social network for labor support (Krishnan and Sciubba 2009; Mekonnen and Dorfman 2013).

Inspired by early anthropological studies (Malinowski 1922; Mauss 1954; Scott 1976), structure and functioning of informal support has become a focus in economic research. The observed forms of support are found to share a number of distinct features. Support is provided between people who know each other well (Foster and Rosenzweig 2001; Fafchamps and Gubert 2007a). Support is typically not confined to a one-time act of assistance but is part of a longer-term arrangement (Kranton 1996; Ligon et al. 2002; Jackson et al. 2012). This arrangement is seldom the outcome of explicit negotiations, rather it is built on an implicit agreement on "unwritten,

but well-understood, rules that specify the level and direction of transfers of goods and services" (Kranton 1996, p.830). Furthermore, the majority of the observed support arrangements appear to be mutual – i.e., each party of the arrangement is expected to provide support. Different motives can explain why people engage in support arrangements (Cox and Fafchamps 2007; Ligon and Schechter 2012): they might derive pleasure by the pure act of providing support; they might be concerned about the welfare of the person they support; they might follow a general custom; yet primarily, people are assumed to be extrinsically motivated and to follow strategic considerations when engaging in support arrangements (Coate and Ravallion 1993; Kranton 1996; Foster and Rosenzweig 2001; Ligon et al. 2002) – i.e., they agree to provide support in the expectation that, "in one form or another, there will be a tangible quid-pro-quo for their present generosity" (Platteau 1997, p.768). As there is no underlying formal contract specifying the terms and conditions which could be legally enforced, other mechanisms are needed to ensure compliance. The mechanism studied most extensively in this context is the threat of 'reversion to autarchy.' When the expected support is not provided, the arrangement is dissolved and the reneging individual needs to deal with future hardships in isolation (Kimball 1988; Coate and Ravallion 1993; Foster and Rosenzweig 2001; Ligon et al. 2002). The effectiveness of this threat is thus directly dependent on the alternative resources individuals have on hand.

1.2 Research Approach

In this thesis, I investigate empirically the relationship between informal support arrangements and access to alternative resources.[1]

Many developing countries have experienced rapid economic development that goes hand in hand with increasing possibilities for the population to cope with hardships individually. Insurance markets have expanded, financial institutions offer saving and loan products designed to meet the demand of the low-income population and with the promotion of mobile banking, people living in remote rural areas have gained access to financial services. How does this development affect informal support arrangements in the villages? Few studies have analyzed the impact of an improvement in individual-level resources on informal support empirically, and these studies come to mixed results (Attanasio and Rios-Rull 2000; Angelucci and De Giorgi 2009; Lin et al. 2014; Dupas et al. 2015).[2] It is crucial to study the relationship between informal support and access to alternative resources in more detail in order to make predictions on how this development will affect the social structure and to specify welfare effects.

This is where my thesis comes in. From three different angles I investigate empirically

[1]The literature uses a number of different terms to describe these support arrangements: mutual insurance (Ligon et al. 2002), informal insurance (Coate and Ravallion 1993), risk sharing (Fafchamps and Lund 2003), risk pooling (Foster and Rosenzweig 2001), reciprocal exchange (Kranton 1996). The more general term "informal support arrangements" yet captures the main features of the observed arrangements well while remaining neutral on the direction of support, the underlying motives and the outcome of the arrangement.

[2]Some studies find an increase in informal support, which they explain by an improvement of the economic situation of the beneficiaries (Angelucci and De Giorgi 2009; Flory 2011; Dupas et al. 2015); while others find a reduction in informal support, which is assumed to be caused by an increase in the value of 'living in autarchy' (Attanasio and Rios-Rull 2000; Lin et al. 2014).

the relationship between informal support within rural communities and households' access to alternative resources. First, I study the structure of mutual support arrangements in a fishing village on the Philippines (Chapter 2). Based on a model of strategic link formation I investigate which factors explain that two households engage in a mutual support arrangement with a focus on the role of households' alternative resources. In a second study I investigate how the willingness to provide support can be affected by the availability of alternative resources, in particular individual insurance (Chapter 3). This is analyzed based on a lab-in-the-field experiment conducted in Cambodia. Different to the first study, the second study analyzes support that is motivated by social preferences, i.e. a concern for the welfare of another person; furthermore, the focus is on the actual provision of support. The last study conceptually links the first two studies. I relate the subjects' support decisions observed in the experiment back to the subjects' engagement in mutual support arrangements in 'real life' (Chapter 4). I thus put the behavior in context to the social structure.

The two countries, the Philippines and Cambodia, are very suitable for the research focus. Both are characterized by a strong economic growth in the last two decades and yet a stark and increasing divide between the rich and the poor (ADB 2009; World Bank 2013b; ADB 2014). In both countries, access to financial services, including insurance, for people with low income is very limited and the social safety net is insufficient (World Bank 2011; Llanto 2015; World Bank 2015).[3] Therefore, the social support network plays an important role to deal with the everyday hazards and risks in particular in rural communities. The two countries thus provide an adequate setting to investigate the structure of informal support arrangements and the interplay with the increasingly available alternative resources.

The first study in this thesis is based on a household survey that I designed and conducted in 30 fishing villages in Western Visayas on the Philippines in summer and fall 2012, covering 476 households. The second and third study are based on a research project conducted together with Susan Steiner. In close cooperation we developed the design of the lab-in-the-field experiment and the household survey. The field research was conducted in 21 villages in North-West Cambodia in summer and fall 2015, covering 1270 households.

1.3 Thesis Outline

The thesis consists of three separate studies. Chapter 2 analyzes the structure of informal support arrangements between households and highlights the importance of households' access to alternative resources. In this chapter, I analyze the likelihood that two households engage in a mutual support arrangement in dependence on the alternative resources they have available to cope with an emergency in isolation. The theoretical framework that guides the analysis assumes quid-pro-quo considerations as motive for the formation of mutual support arrangements. I hypothesize, first, that households only engage in mutual support arrangements if they have insufficient alternative resources on hand and thus face a positive probability of being in need of

[3]Recently, there have been considerable advances by the Philippines government to provide health coverage to the poor, yet with mixed success, as I will discuss in Chapter 2.

support; and second, that two households are less likely to form a mutual support arrangement the more they differ in their respective probability to become needy. I test the hypotheses using census network data from a fishing village on the Philippines. I find that households engage in mutual support arrangements even if they have access to alternative resources, yet that two households are less likely to form a mutual support arrangement the more they differ in their available resources and thus their probability of neediness. Furthermore, I show that the structure of one-sided support arrangements, where support is expected only from one side, differs systematically from the structure of mutual support arrangements. Besides the empirical analysis, this chapter highlights the methodological challenges of analyzing informal support arrangements within a dyadic framework.

Chapter 3 focuses on support that is motivated by social preferences. This chapter is joint work with Susan Steiner. We analyze whether less support is provided to a person who experienced an income loss when this person could have avoided her loss by purchasing insurance; that is whether solidarity is conditioned on the availability of alternative resources. We conducted a lab-in-the-field experiment with 672 villagers from 21 villages in North-West Cambodia. In the experiment, subjects are split in 'providers' and 'recipients.' All subjects receive the same endowment. However, with a probability of 50%, the recipients can lose almost all of their endowment while the providers can keep their endowment. Half of the recipients have the option to purchase insurance, which would cover their loss; the other half do not have this option. The provider is matched with a recipient, to whom the provider can transfer in case the recipient lost. Each provider is asked how much she would transfer in case the recipient had no option of insurance, and how much she would transfer if the recipient had the option of insurance but forewent this option. The difference in the transfer decisions indicate the extent of conditional solidarity. We find a significant reduction in transfers when the recipient had the choice of insurance. Providers transfer on average 30% less when recipients could have purchased insurance. Thus, solidarity is conditioned on the availability on alternative resources. Yet, while on average there is a significant reduction in transfers, we observe considerable heterogeneity in the individual subjects' transfer behavior. 44% reduce their transfers when the recipient had the choice of insurance; yet, 43% transfer the same amount independent of the insurance availability.

In Chapter 4, I analyze this heterogeneity in more detail by incorporating socio-economic information on the subjects which was collected as part of a household survey conducted prior to the experiment. In particular, I relate the providers' transfer behavior in the experiment to their engagement in mutual support arrangements in real life. The focus is on informal exchange of credit, food and labor. I find that subjects, who have large informal exchange networks, tend to condition their solidarity on choice. They punish a recipient's decision not to take up insurance by more than subjects with smaller exchange networks. The results indicate that the tendency to hold others accountable is related to a person's engagement in informal exchange. There are two possible interpretations of the observed correlation. One interpretation is based on an argument of selection: people who show a stronger inclination to hold others accountable are also more likely to engage in informal exchange; potentially because the benefit that they derive from these arrangements is greater than for other people due the higher threat of punishment

they pose. The other interpretation follows a learning argument: people engaged in informal exchange arrangements learn the importance of holding others accountable and punishing non-compliance to sustain cooperation. People with more exposure to informal exchange internalized this notion of accountability and act in accordance also outside these arrangements. With the data on hand I cannot identify which interpretation most adequately explains the observed correlation. Notwithstanding, the findings reflect the complexity of informal support institutions and highlight the value of relating subjects' decisions in experiments to their socio-economic background, to gain insight into conditions and motives that affect decision-making and to derive adequate policy implications.

Chapter 2

The Role of Expected Neediness for the Formation of Mutual Support Arrangements

2.1 Introduction

In many developing countries, the poor have little resources on hand to cope with emergencies. Most are not covered by any type of insurance; and the types of loans offered by financial institutions are mostly not suitable for emergencies. Illnesses or injuries through accidents can have severe long-term consequences, not only due to the foregone income but also due to the health related expenses that are often covered privately. As access to formal risk coping strategies is limited, support from the immediate social network plays a crucial role in dealing with the consequences of shocks. There is substantial evidence for dense inter-household support networks. Households support each other in terms of money, food, shelter or labor assistance (Fafchamps and Lund 2003; De Weerdt and Dercon 2006; Krishnan and Sciubba 2009; De Weerdt and Fafchamps 2011; Ambrus et al. 2014). These support arrangements are commonly assumed to be guided by a principle of 'balanced reciprocity' (Platteau 1997). Provision of support is conditioned on the implicit agreement that the support will be reciprocated should the support providing household be in need of assistance in the future. In the literature, this is often described as (reciprocal) risk-sharing (Foster and Rosenzweig 2001; De Weerdt 2004; Fafchamps and Gubert 2007b; Ambrus et al. 2014). In this study, I use the more neutral term *mutual support arrangement*.

This chapter studies determinants for the formation of mutual support arrangements. Which characteristics explain that household i forms a mutual support arrangement with household j, but not with household k? More specifically, I investigate the role of predicted *neediness*, whereby a household is defined as needy if it must call upon another household for support in case of an emergency. Building on the theory of reciprocal risk-sharing (Coate and Ravallion 1993; Foster and Rosenzweig 2001; Ligon et al. 2002), I develop a theoretical framework that describes mutual support arrangements as bilaterally agreed upon arrangements between agents

who differ in the resources available to cope with a shock in isolation, and thus in their respective probability of becoming needy. I derive two hypotheses. First, an agent is more likely to engage in a mutual support arrangement, if she faces a positive probability to become needy; that is, if she has insufficient resources to cope with an emergency in isolation (Hypothesis 1). Second, a mutual support arrangement between two agents is less likely the larger the difference in the agents' respective probability to become needy (Hypothesis 2).

The empirical analysis consists of two parts. First, I determine which variables predict a household's probability to become needy conditioned on a shock experience. I analyze data that cover detailed information on shock-coping strategies and the role of the village support network for a random sample of 306 households in 22 fishing villages in Western Visayas on the Philippines. I show that household size, wealth and access to sources of support outside the village affect the probability of neediness. Households of small size, low asset wealth and little connections outside the village are significantly more likely to seek for support from their village neighbors. Second, in order to test the two predictions of the theoretical framework I make use of another data set. One of the 22 villages was surveyed completely. Detailed information on the support arrangements between all households residing in the village were elicited, which allows me to construct the complete inter-household support network within the village. Analyzing the structure of the support network by dyadic regression, I show that predictors of neediness indeed affect the formation of mutual support arrangements. Yet, the findings are not univocal. Hypothesis 1 cannot be confirmed. Households with less resources and thus a higher probability of neediness are *not* necessarily more likely to engage in mutual support. On the other side, I find confirmation for Hypothesis 2. The more households differ in variables that determine their probability of neediness the less likely it is that they form a mutual support arrangement. However, not all variables of interest have the predicted effect. In order to address endogeneity concerns, in a next step, I develop a propensity score applying weights that are derived from the larger data set. The score predicts each household's probability of neediness based on exogenous household characteristics. Using the score as predictor for the formation of mutual support arrangements confirms the first results. Households with a similar score of predicted neediness are more likely to form mutual support arrangements, yet a lower score does not necessarily increase the likelihood that a household engages in mutual support. In a final step, I show that results change considerably once the local network structure is taken into account. In the course of the analysis, I demonstrate how results vary depending on the specification of a mutual support arrangement, in particular, depending on whether support links that are reported by one side only are distinguished from support links that are reported by both sides.

This study makes three major contributions. First, it contributes to the broad literature on risk-sharing. While, theoretically, it can be shown that the probability of neediness plays a pivotal role for the formation and sustainability of mutual support arrangements, to the best of my knowledge, this specific aspect has never been analyzed empirically. In this study, I show that predicted neediness can indeed explain part of the structure of the observed support network and might be able to reconcile some of the contradictory findings of previous studies. Second, the study contributes methodologically to the empirical literature on the formation of informal

support arrangements. In many studies, mutual support arrangements are not distinguished from one-sided support arrangements; this is often due to missing data: there is no report by the support arrangement partner, or the underlying survey question does not explicitly ask for the direction of support. Using the structure of the reported support links to differentiate between one-sided and mutual support arrangements I show significant differences in the estimated models depending on whether a distinction between the two types of arrangements is made or not; neglecting this distinction can result in erroneous inference. Finally, this study informs future research of the challenges faced when analyzing link formation in a social network context. Throughout the chapter, I highlight the difficulties researchers are confronted with when analyzing endogenous link formation and the corresponding methodological constraints. Thereby, the study aims to advice future research to apply appropriate techniques in the elicitation and analysis of social networks in the context of risk-sharing.

The remainder of the chapter is structured as follows. In Section 2.2, I provide a brief overview on the relevant theoretical and empirical literature on risk-sharing arrangements and highlight how the present study can address some of the shortcomings of previous studies. In Section 2.3, I first present the theoretical framework and the derivation of the two hypotheses, and then discuss the empirical strategy to analyze the theoretical predictions with network data. The research setting and the data are presented in Section 2.4. In Section 2.5, the empirical model is specified, the results are presented and discussed. Section 2.6 concludes.

2.2 Literature

This study builds on the theoretical and empirical literature that investigates the formation of mutual support arrangements in the context of risk-sharing, and within this field, more specifically, on studies that analyze support link formation in a social network setting. While this literature does not discuss the aspect of predicted neediness explicitly, previous findings with respect to income shock probability and access to alternative resources can be set in direct context.

One of the first theoretical analyses of mutual support arrangements as reciprocal risk-sharing agreements is the paper by Coate and Ravallion (1993). Two risk-averse agents face uncorrelated income streams that vary over time. When income cannot be stored and resources cannot be accumulated over time, the agents have an incentive to agree on an informal risk-sharing contract which specifies monetary transfers in dependence on the income realized. As there is no external enforcement mechanism in place, the agreement must be self-enforcing. One possibility is 'reversion to autarchy:' in case of defection, the agreement is canceled and no future transfers will take place. The authors show that the incentive to defect increases when income streams are correlated. Subsequent papers extend the basic model. It is shown that a risk-sharing arrangement is less sustainable when the value of living in autarchy increases with the access to alternative resources, e.g. due to the introduction of insurance or credit technologies that allow the transfer of resources over time (Ligon et al. 2002; Lin et al. 2014), and when income shocks,

and thus the probability to be in need of support, are persistent over time (Attanasio and Rios-Rull 2000). More recently, mutual support has been analyzed within a network context, where agents can form support arrangements with more than one other agent (Bramoullé and Kranton 2007; Bloch et al. 2008; Jackson et al. 2012; Ambrus et al. 2014). The local network structure can thus affect the stability of the arrangements, which are shown to be more sustainable if reneging on an agreement can result in the loss of multiple support links (Bloch et al. 2008; Jackson et al. 2012). In summary, mutual support arrangements are predicted to be more sustainable, when income streams are uncorrelated, the value of living in autarchy is low as access to alternative resources is limited, when shocks are non-persistent, and the arrangement is embedded in a dense social network.

There is substantial empirical work on mutual support arrangements, yet most authors focus on the outcome of these arrangements – that is the level of income pooling that is achieved (e.g. see Townsend (1994), Udry (1994), Gertler and Gruber (2002), and Fafchamps and Lund (2003)). Few studies analyze the formation process and the determinants for the sustainability of mutual support arrangements. These studies can be broadly split into studies that draw on behavioral experiments (e.g. in Barr et al. 2012; Attanasio et al. 2012; Lin et al. 2014) and studies that analyze household survey data (e.g. in De Weerdt 2004; Fafchamps and Gubert 2007b; Schechter and Yuskavage 2012).

From the experimental literature, two studies are of particular relevance: Attanasio et al. (2012) and Barr et al. (2012). Both studies use risk-pooling games to investigate the formation of mutual support groups in Colombia and Zimbabwe respectively. The games are conducted in rural communities where mutual support arrangements are common. In these games, participants are asked to form groups with other people from their community; they then individually make choices on lotteries, the proceeds of which are shared equally within the groups that have been formed. Treatments vary in the extent sharing is enforced. Attanasio et al. (2012) find that people tend to group with relatives and friends and that they then group assortatively on their risk preferences. Furthermore, the authors find that villagers with similar levels of consumption are more likely to group. Barr et al. (2012) find that groups are typically formed with people that are similar to them in terms of gender and age, but find no effect of wealth or income; that is, people seem not to take other people's household wealth into account when deciding with whom to form a sharing group. A drawback of the experiments is that the arrangements analyzed are formed for a specific one-time purpose; they lack the repeated game characteristic of real-life support arrangements, thus the implications for the formation process of continual support arrangements are limited.

Few studies analyze the determinants for support arrangements by using network data elicited through household surveys. Indeed, this requires very specific data which ideally cover socioeconomic information for all potential support partners within a predefined network. Importantly, this information should allow the researcher to disentangle outcomes and drivers of the formation of support arrangements. Furthermore, as risk-sharing is per definition not a unilateral arrangement, the data should contain independent statements of each household on its support links

which in addition distinguish the direction of the support. Few data sets fulfill these requirements. To the best of my knowledge there are only three studies, which investigate support links in the context of risk-sharing explicitly drawing on data sets which partly fulfill the above listed requirements. In the following, these three papers are discussed in more detail, since they are similar to the present study with respect to the underlying data structure and the estimation approach, but also have a number of short-comings that the present study aims to overcome.

De Weerdt (2004) investigates support links in a village in Tanzania where all households residing in the village were surveyed. Each adult was asked to list people in the village " *(...) [she] can personally rely on for help and/or that can rely on [them] (...)*" (p. 201). For the main specification, De Weerdt defines a support link connecting two households as existing if at least one member of one of the two households lists at least one member of the other household; the support link is analyzed as undirected; it is not specified which of the two households reports the link, nor whether a reported link is reciprocated. De Weerdt analyzes the determinants of link formation based on dyadic regression analysis, including pair specific characteristics (in particular, differences in household characteristics), yet not level effects. He finds that the likelihood of a support link between two households increases when they are related, live close by and share the same religious affiliation. In contrast to the theoretical predictions, De Weerdt finds that households with a larger occupational overlap as well as similar levels of wealth in terms of livestock and landholding are *more* likely to be linked.

Fafchamps and Gubert (2007b) study support networks of a random sample of households in four villages on the Philippines. Each household is asked to name up to four people "*(...) on which it could rely on in case of need or to whom [it] gives help when called upon to do so*" (pp. 331). Different to De Weerdt (2004), the authors analyze the determinants for a directed link; that is they differentiate a link from i to j, which exists if i names j, from a link from j to i, which exists if j names i. As not all households in a community were surveyed, only links among the sampled households are considered. The analysis is conducted using dyadic regression analysis accounting both for differences in household characteristics as well as level effects. Furthermore, the authors develop a novel method for correcting the standard errors to account for dyadic correlation (more on this in Section 2.3.2.1). Similar to DeWeerdt, the authors find that kinship and geographic proximity are important factors determining link formation and that households with a higher correlation in income are more likely to link. Contrary to De Weerdt, they find links to be more likely between poor and rich households than between households of similar wealth.

One major drawback of both studies is that, while the authors' focus is on risk-sharing, which postulates mutuality, due to the phrasing of the survey question the elicited support link can refer to *both* mutual and one-sided support arrangements; this renders it difficult to interpret the results in the context of risk-sharing.[1] Schechter and Yuskavage (2012) partly address this issue. They analyze support links based on a sample of households from 15 villages in rural Paraguay. Households are asked in two separate questions from which household they could

[1]While the analysis in Fafchamps and Gubert (2007b) is conducted for a 'directed' link, the direction is based on the report (i.e., 'who names whom') not the flow (i.e., 'who supports whom').

borrow a specified amount of money if needed, and which household would come to them to borrow. A mutual support link is defined as existing, if a household lists another household in both questions. An one-sided support link is defined as existing, if the other household is listed only once. Using multinomial logit estimation, the authors simultaneously estimate the likelihood of the existence of a mutual and of a one-sided link among the surveyed households.[2] They find that one-sided support links are more likely from wealthier to poorer households, while mutual support links are more likely between two wealthier households, where wealth is measured by the value of land, animals and tools owned.

In summary, the empirical literature on the formation of mutual support arrangements cannot clearly confirm the theoretical predictions of the basic risk-sharing framework. While the theoretical framework suggests that arrangements are more sustainable if income streams are uncorrelated, empirical studies find the opposite to be the case; households with a larger activity overlap and correlated income streams are more likely to form a mutual support arrangement (De Weerdt 2004; Fafchamps and Gubert 2007b). With respect to wealth and access to alternative resources, evidence is mixed. Some studies find that households with similar wealth level are more likely to link with each other (De Weerdt 2004; Schechter and Yuskavage 2012), other find the opposite to be the case (Fafchamps and Gubert 2007b) or no effect at all (Attanasio et al. 2012; Barr et al. 2012).

In this study I suggest two explanations for the seemingly contradictory findings. First, the concept of predicted neediness might explain why households with correlating income streams are more likely to link. Income correlation and activity overlap can indicate a similar probability of neediness. As long as income shocks are not directly polychoric this similar probability of neediness can reduce the incentive to defect and enhance the sustainability of a mutual support arrangement. In the next section this mechanism is explained in more detail. Second, the empirical specification of support arrangements in the studies mentioned above partly mixes one-sided and mutual support arrangements. None of the studies distinguishes between reciprocated (i.e., i names j and j names i) and unreciprocated links (i.e., i names j but j does not name i). This can result in erroneous inference, as will be shown further below.

2.3 Theory and Method

In the following, I first discuss the theoretical framework based on which the two hypotheses are derived that guide the empirical analysis (Section 2.3.1). The theoretical framework builds on the model of favor exchange by Jackson et al. (2012) but is modified to reflect the probability of neediness.[3] I then present the estimation strategy to test the hypotheses within a dyadic

[2] Note that instead of 'mutual and one-sided support links,' the authors talk about 'reciprocated and unreciprocated links.' In a network context, this term can be confusing as only the report from one side is used and not from both.

[3] Jackson et al. (2012) focus on the properties of stable networks in the context of favor exchange. In their main specification they do not allow for asymmetric probabilities; however, they later show the implications of asymmetric payoff functions, expanded on in an online appendix to their paper.

framework (Section 2.3.2). Both for the theoretical model and the estimation strategy, I discuss the underlying assumptions and their implications for the econometric specification as well as for the interpretation of the results.

2.3.1 Theoretical Framework

There are two selfish agents i and j that live over an infinite number of periods. In each period, an agent faces the probability π of a negative income shock with $0 < \pi < 1$. The two agents can agree to form a mutual support arrangement. If a support arrangement is formed, then in case i is in need of support, j provides support to i; and in case j is in need of support, i provides support to j. For simplicity, it is assumed that providing support costs the provider a fixed amount c while the value of receiving support is v, with $v > c$; thus engaging in mutual support over time is ex-ante pareto-efficient. Let p_i represent the probability that i is in need of support in a given period, i's *probability to become needy*. More specifically, we assume $p_i = \pi(1 - r_i)$, where r_i is the probability that agent i can cope with the shock individually, with $0 \leq r_i \leq 1$. r_i depends on the alternative resources i has available to cope with the shock individually. While π is fixed, agents differ in r_i and thus their likelihood of neediness p_i.

i and j agree to form a mutual support arrangement if for both agents the expected utility of the arrangement is non-negative:

$$U_i = \frac{\delta\big(p_i\, v - p_j\, c\big)}{1-\delta} \geq 0 \qquad \text{and}$$
$$U_j = \frac{\delta\big(p_j\, v - p_i\, c\big)}{1-\delta} \geq 0$$

where δ is a discount factor by which the agents discount their payoffs over time, with $0 < \delta < 1$. Agents only engage in a mutual support arrangement if they cannot cope with the shock individually, that is if $r_i < 1$ and $r_j < 1$. As commitment is limited and there is no external enforcement mechanism in place, the arrangement must be self-enforcing. Compliance can be enforced through the threat of autarchy: once an agent deviates, the support arrangement will be dissolved and the agents have to deal with future shocks in isolation. Thus, mutual support is not sustainable if the cost of providing support when asked exceeds the discounted benefit of continuing the arrangement. This condition is expressed in Equation 2.1:

$$c > \frac{\delta\big(p_i\, v - p_j\, c\big)}{1-\delta} \tag{2.1}$$

Ceteris paribus, the incentive to deviate increases the larger $|p_i - p_j|$. That is, the larger the difference in the agents' likelihood of neediness, the less likely it is that an arrangement is sustained.
Based on this framework, the following predictions are derived which can be tested empirically:

Hypothesis 1
An agent only engages in a mutual support arrangement if her alternative resources to cope with

a shock are insufficient and she thus faces a positive probability of becoming needy.

Hypothesis 2
Two agents are less likely to maintain a mutual support arrangement the larger the difference in their respective probability to become needy.

The model is built on a number of assumptions that have important implications for the empirical analysis and the interpretation of the results.

First, the model assumes v and c to be fixed and r_i to be stable over time. However, in reality v and c are likely agent specific, e.g. dependent on an agent's wealth. More importantly, in reality, resources are depletable and, moreover, can be decision parameters of an agent, who, for example, can decide to invest in her resources in dependence on the mutual support arrangement she formed. For the empirical specification this potential endogeneity needs to be considered and access to alternative resources should be proxied by variables that are exogenous to the mutual support arrangement.

Second, resources are assumed to be observable. However, only in a small community context, as it is the case for the present study, it does seem plausible to assume that resources are observable or, at least, that an agent can form rational expectations of another agent's resources based on observable characteristics. In other contexts, resources are less observable and agents might even have the incentive to send untruthful signals about their level of resources (Genicot 2015; De Weerdt et al. 2015). Moreover, the probability to become needy likely depends less on resources available today and more on the resources expected to be available in the future. While this information might be inferable by other agents in the community, it is difficult to be observed by researchers. We need to assume that the resources which can be observed today serve as adequate predictors for future resources.

Third, the model makes the implicit assumption that an agent prefers to employ her alternative resources first before turning to others to ask for support. This might not be the case in reality, in particular as the use of personal resources is unlikely to be cost-free. In the present study, I can provide some evidence that households are indeed inclined to employ alternative resources first before turning to others (see Section 2.5.1.2); yet, this does not need to be the case in other settings, and likely depends on the social and cultural context.

2.3.2 Empirical Method

In order to analyze the two hypotheses empirically, in the following the two agent framework of mutual support arrangements is set in a network context and a dyadic regression model is derived (Section 2.3.2.1). I then expand the basic model to account for one-sided support arrangements (2.3.2.2).

2.3.2.1 Estimating the formation of mutual support arrangements in a network context

I assume a set of agents $N = \{1, ..., n\}$ living in a community of size n. Agents are linked by different types of relationships. I focus on mutual support arrangements which are assumed to be bilateral agreements between a pair of agents as described above. The network of mutual support arrangements is represented by the graph ς, where for each pair of agents (ij), $\varsigma_{ij} = 1$ if there exists a mutual support arrangement between i and j, and $\varsigma_{ij} = 0$ else, with $i, j \in N$ and $i < j$. Thus ς is an *undirected* network with $\frac{n \cdot (n-1)}{2}$ possible links.

The expected utility of a mutual support arrangement between agents i and j within a support network can be described as

$$U_i(\varsigma_{ij} = 1) = \frac{\delta\big(p_{ij}\, v - p_{ji}\, c\big)}{1 - \delta} \geq 0 \qquad \text{and}$$
$$U_j(\varsigma_{ij} = 1) = \frac{\delta\big(p_{ji}\, v - p_{ij}\, c\big)}{1 - \delta} \geq 0$$

where p_{ij} is the probability that i asks j for support and p_{ji} the probability that j asks i.

The mutual support arrangement is based on a bilateral agreement. A support arrangement ς_{ij} between two agents i and j is only formed if the utility each of the two agents derives from the support network is at least as large when the support arrangement is formed, compared to the utility the agents derive from the network when the arrangement is not formed.[4] That is:

$$\varsigma_{ij} = 1 \quad \textit{if} \quad [U_i(\varsigma_{+ij}) \geq U_i(\varsigma_{-ij}) \ \textit{and} \ U_j(\varsigma_{+ij}) \geq U_j(\varsigma_{-ij})],$$

where ς_{+ij} describes the network of mutual support arrangements including the arrangement between i and j, while ς_{-ij} is the network of mutual support arrangements if there is no mutual support arrangement between i and j.

I assume that the benefits from an additional arrangement depend on a vector of observable characteristics, which include characteristics that describe each agent's probability of neediness, and a residual.[5]

$$U_i(\varsigma_{+ij}) - U_i(\varsigma_{-ij}) = \alpha + X'_{ij}\beta + \epsilon_{ij}$$
$$U_j(\varsigma_{+ij}) - U_j(\varsigma_{-ij}) = \alpha + X'_{ji}\beta + \epsilon_{ji}$$

We can thus write the likelihood that a pair of agents (ij) agrees on a mutual support arrangement as

$$P(\varsigma_{ij} = 1) = P(-\epsilon_{ij} \leq \alpha + X'_{ij}\beta \quad \textit{and} \quad -\epsilon_{ji} \leq \alpha + X'_{ji}\beta) \ .$$

The true value of ς_{ij} is unobserved. But suppose we have from each agent i, with $i = (1, ..., n)$, an

[4] This equilibrium concept of link formation is known as pairwise stability (Jackson and Wolinsky 1996).

[5] The following specification is based on the approach suggested by Comola and Fafchamps (2014) to estimate bilateral link formation.

independent report on her potential sources of support within her community, with s_{ij} indicating the reported support link from i to j with $i \neq j$. More specifically, $s_{ij} = 1$ if i names j as a source of support in times of need and $s_{ij} = 0$ otherwise; and $s_{ji} = 1$ if j names i as a source of support in times of need and $s_{ji} = 0$ otherwise. Thus, different to ς, s is a *directed* network with $n \cdot (n-1)$ possible links. s can be used to proxy the unobserved structure of the support arrangement network ς.

In a first step, it is assumed that each reported support link represents a mutual support arrangement; that is a mutual support arrangement exists if $s_{ij} = 1$ or $s_{ji} = 1$. We can then estimate

$$P(\varsigma_{ij} = 1) = P(s_{ij} = 1 \ or \ s_{ji} = 1) = \frac{e^{\alpha + X'_{ij}\beta}}{1 + e^{\alpha + X'_{ij}\beta}} \tag{2.2}$$

by maximum likelihood under the condition that there is degree variation in the characteristics across dyads; that is, $X_{ij} \neq X_{ji}$ for at least some dyads (Fafchamps and Gubert 2007b).

The estimation strategy is based on a number of assumptions which have, in addition to the assumptions of the theoretical model discussed above, important implications for the empirical specification. I briefly outline them in the following; I address them in more detail in the remainder of the chapter.

First, Specification 2.2 does not allow for interdependencies in link decisions; the strong assumption has to be made that the network structure cannot affect link specific utilities (Chandrasekhar 2016). In particular, the theoretical framework assumes link-specific probabilities, and thus the probability that i asks j for support might depend on the number of support arrangements i has in place; yet, interdependency between links cannot be estimated with a pairwise regression model. I will discuss this and other types of link-interdependencies as well as approaches to address these issues in Section 2.5.1.1 and Section 2.5.3.3.

Second, building dyadic regression analysis on individual choices requires two further assumptions. a) Separability of the utility functions; that is, the utility of a network is assumed to be the sum of the utility derived from each link. b) Symmetry for the case of undirected links; that is, the additional value i derives from being linked with j is assumed to be the same as the additional value j derives from being linked with i (Bramoullé and Fortin 2010). The estimated model needs to be specified accordingly; more specifically $(X_i + X_j)$ and $|X_i - X_j|$ should be included as regressors for the analysis of undirected links, and X_i, X_j and $|X_i - X_j|$ for the analysis of directed links. The specification of the regressors will be addressed in more detail in Section 2.5.2

Third, undirected link formation assumes mutual consent. If an agent i lists another agent j as a source of support, without j being aware of this role, the choice foundation of the estimation approach would be put into question; this would challenge the interpretation of the results (Comola and Fafchamps 2014). I will come back to this issue when specifying the variables in Section 2.5.1.1.

Fourth, the error term structure in Specification 2.2 needs to allow for correlation across

observations. Error terms across observations can be correlated in at least three distinct ways: for $i, j, k \in N = (1, ..., n)$, $E[u_{ij}, u_{ik}] \neq 0$, $E[u_{ik}, u_{jk}] \neq 0$ and $E[u_{jk}, u_{ij}] \neq 0$ (Cameron and Miller 2014). As shown by Fafchamps and Gubert (2007b) this can result in inconsistent standard errors. The authors propose a network corrected covariance matrix to account for the correlated standard errors:

$$AVar(\hat{\beta}) = \frac{1}{n-m}(X'X)^{-1}\left(\sum_{i=1}^{n}\sum_{j=1}^{n}\sum_{k=1}^{n}\sum_{l=1}^{n}\frac{I_{ijkl}}{2n}X_{ij}u_{ij}u'_{kl}X_{kl}\right)(X'X)^{-1} \tag{2.3}$$

where β is the vector of coefficients, n is the number of observations, m is the number of regressors, X is the matrix of all regressors, X_{ij} is the vector of regressors for the dyadic observation ij, and $I_{ijkl} = 1$ if $i = k, j = l, i = l$ or $j = k$, and 0 otherwise. In the regression analysis, standard errors need to be corrected accordingly.[6]

Finally, it is not straightforward how to use independent reports on undirected links, if these reports mismatch. This issue will be discussed in the following Section 2.3.2.2 in more detail.

2.3.2.2 One-sided versus mutual support arrangements

How to proceed if reported support links do not correspond with each other – i.e., $s_{ij} \neq s_{ji}$? This is unproblematic as long as all reported support links can be assumed to represent mutual support arrangements; then $s_{ij} \neq s_{ji}$ is due to underreporting, and hence we can assume $\varsigma = 1$ if $s_{ij} = 1$ or $s_{ji} = 1$. However, it is not obvious that the motive of risk-sharing explains all observed support arrangements. Indeed, there is considerable evidence for one-sided support arrangements that follow quite different mechanisms: an agent might provide monetary or in-kind support to ensure the political endorsement by the recipient, to contain unwanted behavior such as theft, to gain social approval by the community or due to fairness concerns (e.g. see Fafchamps (1992), Platteau (1995), Schechter (2007), and Ligon and Schechter (2012)). The provided support is expected to be reciprocated in a different domain, or not at all. In most one-sided support arrangements, expected neediness plays a different role than in mutual support arrangements: the recipient of the support is typically assumed to be resource constrained, while the provider is assumed to have access to sufficient alternative resources; and thus, contrary to the case of mutual support arrangements, a one-sided support arrangement should expected to be *more* likely the larger the difference in expected neediness. This suggests that, when analyzing the implications of expected neediness, it is important to distinguish mutual from one-sided support arrangements.

Let us denote $\dot{\varsigma}$ as the undirected support arrangement network in a community with n agents with $\frac{n\cdot(n-1)}{2}$ possible links. $\dot{\varsigma}$ includes both one-sided and mutual support arrangements. $\dot{\varsigma}_{ij}$ describes the type of support arrangement a pair of agents (ij) have agreed upon, with

[6]There are alternative approaches: Udry and Conley (2004) include individual fixed effects, and Barr and Genicot (2008) use a quadratic assignment procedure (QAP), where standard errors are estimated based on permutations of the data set to account for interdependence of the observations. However, when the number of nodes (i.e., in our case the number of households) is small the dyadic corrected standard errors is considered most suitable (Cameron and Miller 2014).

$i, j \in N = (1...n)$ and $i < j$. A support arrangement can have three outcomes. It might be mutual; that is, i and j agreed that j supports i if i experiences an income shock and i supports j if j experiences an income shock (in this case let $\dot{\varsigma}_{ij} = 3$); but it can also be one-sided: i and j agreed that i supports j if j experiences an income shock but j is not expected to support i in case of an income shock, though i might be compensated by other means, ($\dot{\varsigma}_{ij} = 2$); or vice versa, i and j agreed that j supports i but i is not expected to support j ($\dot{\varsigma}_{ij} = 1$).

If we observe a support network s that is directional not only with regard to who names whom but also with regard to the flow of support, we can use the structure of s to proxy one-sided and mutual support arrangements.

To distinguish unobserved support arrangements from the reported support links, for the remainder of this chapter the following terminology is used.

The unobserved network of support arrangements can contain

- *One-sided Support Arrangements*,
  i and j agreed that j supports i in case of emergency but no explicit agreement has been made that i supports j (i.e., $\dot{\varsigma}_{ij} = 2$), or vice versa (i.e., $\dot{\varsigma}_{ij} = 1$), and
- *Mutual Support Arrangements*,
  i and j agreed that j supports i in case of emergency and that i supports j (i.e., $\dot{\varsigma}_{ij} = 1$).

The network of reported support links consists of

- *Unreciprocated Support Links*,
  i names j as a source of support, but j does not name i (i.e., $s_{ij} = 1$ and $s_{ji} = 0$) or vice versa (i.e., $s_{ij} = 0$ and $s_{ji} = 1$), and
- *Reciprocated Support Links*,
  i and j name each other as source of support (i.e., $s_{ij} = 1$ and $s_{ji} = 1$).

An unreciprocated support link can be used as a proxy for a one-sided support arrangement and a reciprocated support link as a proxy for a mutual support arrangement.

Which type of support arrangement two agents form is still based on a bilateral agreement; that is, a specific support arrangement $\dot{\varsigma}_{ij}$ between two agents i, j is only formed if the utility each agent derives from the support arrangement is weakly positive and there is no other type of support arrangement which *both* agents would prefer. In particular, it is assumed that two agents form a mutual support arrangement $\dot{\varsigma}_{ij} = q$, with $q = (1, 2, 3)$, if the utility each agent i and j derives from the support arrangement network including this specific arrangement $\dot{\varsigma}_{ij} = q$ is at least as large as when a different type of arrangement or no arrangement would be formed.

Specifically, for $q = (1, 2, 3)$

$$\begin{aligned}\dot{\varsigma}_{ij} = q \;\; &\textit{if}\,[U_i(\dot{\varsigma}_{+\dot{\varsigma}_{ij}=q}) \geq U_i(\dot{\varsigma}_{+\dot{\varsigma}_{ij}=l}) \;\; \textit{and} \;\; U_j(\dot{\varsigma}_{+\dot{\varsigma}_{ij}=q}) \geq U_j(\dot{\varsigma}_{+\dot{\varsigma}_{ij}=l})],\\ &\quad \textit{for all } l = (0, 1, 2, 3) \textit{ with } l \neq q;\\ \dot{\varsigma}_{ij} = 0, \;\; &\textit{else.}\end{aligned}$$

Moreover, it is assumed that i and j can be linked by at most one type of support arrangement $\dot{\varsigma}_{ij}$.

The reported support network s is used to proxy $\dot{\varsigma}$, where $P(\dot{\varsigma}_{ij} = 3) = P(s_{ij} = 1 \; and \; s_{ji} = 1)$, $P(\dot{\varsigma}_{ij} = 2) = P(s_{ij} = 0 \; and \; s_{ji} = 1)$, $P(\dot{\varsigma}_{ij} = 1) = P(s_{ij} = 1 \; and \; s_{ji} = 0)$ and $P(\dot{\varsigma}_{ij} = 0) = P(s_{ij} = 0 \; and \; s_{ji} = 0)$. Then the likelihood that a pair of agents (ij) agrees on the support arrangement $\dot{\varsigma}_{ij} = q$ can be jointly estimated through multinomial logit via maximum likelihood as

$$P(\dot{\varsigma}_{ij} = q) = \frac{e^{\alpha + X'_{ij}\beta_q}}{1 + \sum_{l=1}^{3} e^{\alpha + X'_{ij}\beta_l}} \quad for \; q = (1, 2, 3). \tag{2.4}$$

For identification the set of coefficients of $\dot{\varsigma}_{ij} = 0$ is set to zero; this choice serves as the base category. The above raised points of caution related to dyadic regression analysis of binary choice models apply also for the estimation of multiple choice models. In particular, the coefficients need to be constrained to fulfill symmetry requirements and the standard errors need to be corrected to account for correlation across observations.

2.4 Data

The data used for the analysis stem from a unique data set from a household survey that I conducted in 30 fishing villages in the region Western Visayas on the Philippines. In the following, I first explain the research setting in more detail (Section 2.4.1), before turning to the description of the data, in particular the socioeconomic characteristics of the survey respondents (Section 2.4.2) and their support network (Section 2.4.3).

2.4.1 Research Setting

The household survey was conducted from August to October 2012.[7] The focus of the survey was the use of financial services and the structure of the social network. In 29 of the 30 villages a small, randomly drawn sample of on average 14 households was surveyed, covering around 15% of each village's population. One village, the village Maramig, was surveyed completely. That is, all 65 households which were residing within the village boundaries at the time of data collection were surveyed, covering in total 228 people.[8]

The household survey was typically conducted with the head of each household. The survey covered socioeconomic characteristics of all people who resided in the household at the time of the survey, including access and use of formal financial services, housing characteristics, as well as detailed questions on the informal support networks within and outside the community. For

[7] For the data collection, I accompanied a research team from the University of Mannheim that conducted behavioral experiments in the 30 villages. For more detail on these experiments as well as on the sampling strategy of the villages see Landmann and Vollan 2016.

[8] Officially, the village consists of 69 households. However, at the time of the survey, two households had merged (a woman had moved back to her parents after her husband died) and two households (one single-person household and one family) had moved for temporary work to Manila. One single-person household lives outside the village in the forest and was not reachable during the entire time of the survey. For the following analysis, the data are adjusted so that the latter three households are treated as residing outside the village.

some analyses, I revert to the larger data set, which covers 22 out of the 30 surveyed villages.[9] Yet, for the main analysis, I exclusively focus on the data from Maramig.

Maramig is situated on the main island Panay, the sixth largest island on the Philippines. The next town with a market is 10 km away, reachable by bus. In this region, a large proportion of the population is very poor; in 2012, 31% of the population in Antique was estimated to live below the poverty line (National Statistical Coordination Board 2013). Many people are self-employed and most economic activities are not formalized. But the conditions are changing. Financial institutions have started to expand their services to rural areas; and, with the building of new roads, which facilitates transport to urban centers, formal sector work has grown in importance. Furthermore, catching up with the general trend on the Philippines, work migration has become very common, even in more remote areas. Many young men and women seek temporary employment abroad (as so-called 'Overseas Filipino Workers,' OFW) or in the Marine. The money that is sent home has become an important source of income for the remaining families and improves their purchasing power.[10] Despite these changes, informal risk-sharing institutions and inter-household support arrangements still play a central role: the majority of the surveyed households name assistance from relatives and neighbors as the main strategy to cope with emergency situations. Also, traditional community support schemes assume an important function, such as 'abuloy,' a practice where in case of a death in the village, all inhabitants are asked to donate money and food for the family of the deceased, each contribution being meticulously documented.

The prominent role that informal support arrangements play for the villagers' day-to-day risk management, and furthermore, the fact that information is available on all households residing within the village's boundaries make the data from Maramig ideal to analyze the mutual support network.

2.4.2 Socioeconomic Characteristics

Table 2.1 provides summary statistics of the most important socioeconomic characteristics of the households residing in Maramig (for the socioeconomic characteristics of the respondents of all 22 villages refer to Table A.1 in Appendix A.1.1). As is common in the region, a household is generally formed by the nuclear family, thus household size is comparably small; a household has on average 3.5 household members, not including those members that temporarily migrated at time of the survey. The majority of the households are headed by men. 40% of the household heads have high school education, however one out of four household heads has no basic education. That is, they either never went to school or dropped out before finishing elementary school.

Most households in the village are connected through family: on average, a household is

[9] Only for 22 villages is data available to me in unconstrained form. The data of the other 8 villages have not yet been digitized.

[10] Over one million Filipinos leave the country every year to work abroad. The estimated number of Overseas Filipino Workers in 2012 was 2.22 million, around 2.4% of the overall population; remittances accounted for about 9% of the GDP (Philippine Statistics Authority 2013). Nevertheless, work migration is generally not an option for the very poor, as the so-called placement agencies, through which most of the work migration is organized, demand high fees for their services.

Table 2.1: Household Characteristics; Maramig

	mean	sd	min	max	median	count
Household size	3.46	1.71	1	8	3	65
Female head	0.35	0.48	0	1	0	65
Head has no basic education	0.28	0.45	0	1	0	65
Head completed high school	0.40	0.49	0	1	0	65
No. of family hh within village	9.46	6.51	0	25	10	65
No. of family hh outside village	3.17	2.83	0	15	3	65
% of adults working	0.57	0.35	0	1	.5	65
% of adults working outside village	0.10	0.24	0	1	0	65
Covered by social security	0.20	0.40	0	1	0	65
Fishing as main income source	0.22	0.42	0	1	0	54
Farming as main income source	0.41	0.50	0	1	0	54
Household income last month (PHP)	14,919	43,671	160	330,975	4,000	65
Asset Wealth	0.39	0.20	.0041	1	.36	65
OFW exists	0.12	0.33	0	1	0	65
Remittances recipient	0.57	0.50	0	1	1	65
Amount remittances last year (PHP)	34,388	60,007	2,000	312,000	18,000	37
Coop member	0.34	0.48	0	1	0	65
Bank account	0.05	0.21	0	1	0	65
MFI Member	0.03	0.17	0	1	0	65
Health insurance	0.62	0.49	0	1	1	65
Informal borrowing and lending	0.66	0.48	0	1	1	65
Observations	65					

Surveyed 65 households, covering 225 household members; hh - households; PHP - Philippine Pesos.
*) Income from last month; includes salary, proceeds from self-employment, remittances, loans, public assistance, pensions, payouts from savings and other income (such as gambling).

related to nine other households in the village; in contrast, the number of households outside the village a household is related to and in touch with is much smaller. Half of the adult household members (on average 57%) worked during the last month, most of the others reportedly stayed at home doing domestic work. The vast majority of the employment is within the village, only 10% of the adult household members work outside the village. While Maramig is traditionally a fishing village, only 22% of the 54 households, where at least one household member is working, report fishing as a main income source; more people work as farmers. The average last month's household income is 14,919 PHP, or 3,183 PHP per capita, which is around 76 USD.[11] The income distribution is highly skewed, with almost 50% of the households having less than 4,000 PHP income per month which amounts to less than 32 USD p.c. An important source of income are transfers from family or friends living outside the village (i.e., remittances).

57% of the households report receiving some form of regular transfers from outside the village, mainly from former household members that work in larger cities in other districts. In one out of ten households, at least one former household member is working on the sea or abroad. The average amount that a remittances receiving household received over the last year is not negligible, about 34.000 PhP which is more than twice the average monthly household income. In fact, eight households report having no income besides the remittances they receive.

As most of the reported income is highly irregular and income reports are not always reliable, as a measure for wealth an asset index is derived, using polychoric principal component analysis (Moser and Felton 2007). The asset index is derived on the basis of the larger data set of the 22 villages and includes variables that describe asset ownership and housing characteristics. Variables are chosen based on the proportion of variance they explain. Weights are assigned using the first component, and the index is standardized to be between 0 and 1. The index is checked both for internal and external validity (for details on the derivation of the index and the validity checks see Appendix A.1.2).

Access to financial products is still limited in Maramig. The majority of households have access to very basic financial services through a local cooperative located in the next town, which offers loans to its members for education and investment purposes. There are three state-run social security systems on the Philippines (SSS, GSIS and Pag-IBIG) which provide pension schemes but also credit lines for employees in the private and public sector. In Maramig, in one out of five households, at least one household member is covered by such a scheme. After the cooperative, these schemes form the second most important source for loans. Use of formal banking services is much rarer. Only in three households does a household member have a deposit account with a commercial bank while only two households are registered as members of a microfinance institution. Most households do not fulfill the eligibility requirements of the banks; and microfinance institutions are not yet as developed as in other regions. Therefore, informal lending plays an important role: 66% of the respondents state that their household borrowed or lent money to someone from the village during the last month. Health insurance has gained importance only recently due to a government initiative that aims to reach universal

[11] At the time of the survey, 1 USD was worth around 42 Philippine Pesos, the PPP conversion factor was 17.88 in 2012 (World Bank 2013a).

health coverage through the state run program PhilHealth. PhilHealth is a contribution based insurance scheme, but subsidizes the contribution for the indigent people. In Maramig, 85% of those households that are covered through PhilHealth do not pay for it. Still, health insurance is barely used. When asked about the major strategies to cope with health shocks, of those insured only 30% name the insurance, while 54% name monetary support from friends, neighbors or relatives.

2.4.3 The Support Network

The core of the survey is a social network questionnaire. Each respondent was asked to provide a list of households ('the alter household'), she would consider as close to her household ('the ego household'). No limitations were made on the number of names the respondent could list, but each alter household could be named only once. The respondent was then asked to further specify the relationship to each alter household. In particular whether they were related, friends or neighbors, the type of family relationship if applicable, how long they knew each other and where the alter household resided. In case the alter household was reported to reside in the same village, the respondent was asked to identify the household on a household roster of the village that was presented to her, which allowed matching the households directly. The identification was later verified with one of the village elderlies.[12]

Table 2.2: Summary Statistics of the Support Links in Maramig

	Total	By Household		
	no. of links	mean	min	max
All links	345	5.31	0	11
within Village	236	3.63	0	10
relative	196	3.02	0	10
friend	22	0.34	0	3
neighbor	18	0.28	0	3
outside Village	109	1.68	0	7
relative	101	1.55	0	7
friend	8	0.12	0	2

Reported support links within the village.

Based on the report of the 65 surveyed households.

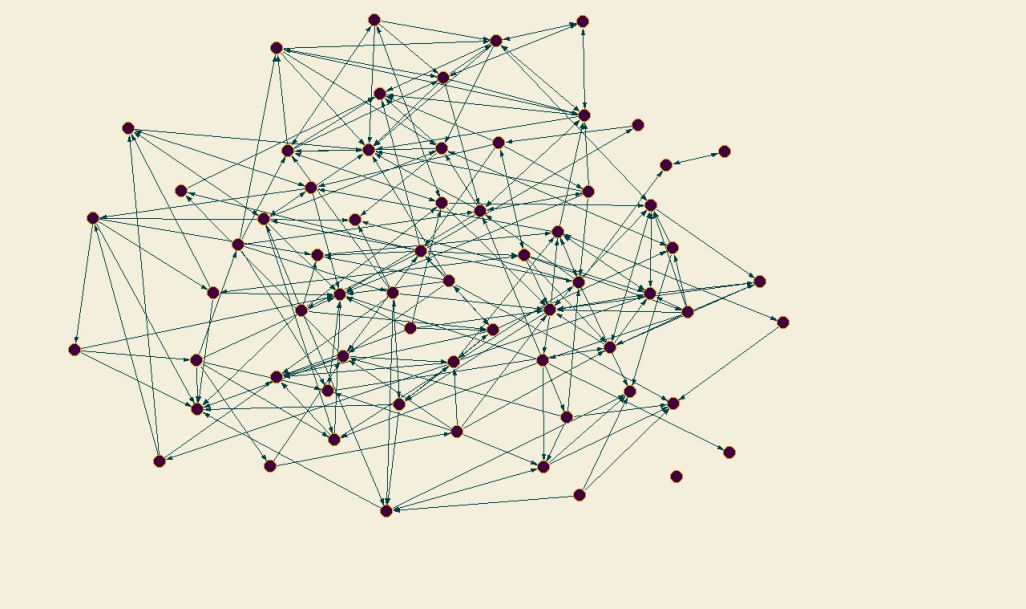
Different types of transfer and support relationships with each alter household were elicited. In this study, I focus on the elicited information on support arrangements.[13] For each of the alter

[12] I am not aware of other studies that have proceeded in a similar manner. Typically, matching is done afterwards by matching the reported names with the names on lists, which can lead to a substantial loss of observations (e.g. see Fafchamps and Gubert (2007b), Comola and Prina (2014), and Banerjee et al. (2013)). Admittedly, the procedure of presenting a household list is only feasible in villages with a small number of households.

[13] The other type of elicited relationships are the following. First, for each alter household the respondent was asked to specify frequency and size of larger monetary transfers provided and received during the last year. Second, focusing on the last 4 weeks, the respondent was asked to indicate whether and how often different specified types of transfers (small amounts of money, food, other in-kind goods) or support (labor services or job hints) had taken place between her and the alter household. Third, the respondent was asked to specify if

households listed, the respondent was asked '*Would these people help you if you/the main income provider would turn very ill and would not be able anymore to earn income and in addition you would need to cover the medical expenses?*' Furthermore, '*Would you ask these people for help?*' For these questions, respondent could respond '*Immediately*,' '*After some hesitation*,' '*Only in extreme emergency situations*' or '*Never*.' I define a support link as existing, if a respondent answered '*Immediately*' for both questions. The support link data form the core of the latter analyses. Advantages and potential shortcomings of this measure are discussed in more detail in Section 2.5.1.1.

Table 2.2 provides summary statistics on the reported support links in Maramig. On average, each household reported five support links, of which over two thirds are within the village. The majority of the reported support links are with relatives. Only around 17% of the reported support links within the village are with unrelated friends or neighbors. This is not surprising given that households are related on average to ten other households within the village.



Reported support links within the village. Based on the reports of the 65 surveyed households.

Figure 2.1: Network of Reported Support Links, directed (map drawn by Pajek)

The resulting network of the support links within Maramig is depicted in Figure 2.1. Each node represents one household. The network is depicted as *directed*, that is the report of each household constitutes a link, and the direction of the report is indicated by the arrow pointing towards the household that is named as a source of support. In case of reciprocated links (i.e., both household name each other as a source of support) the arrow points in both directions. Support between households is common. 55 of the 65 households name at least one other household from the village as a potential source of support. For 37 of these households at least one

and how the alter household had provided support during emergency situations the respondent's household had experienced during the last 3 years.

of the reported support links is reciprocated. 62 households are named as a potential source of support by at least one other household. One household neither names any other household as source of support nor is named by any other household; this household forms an isolate in the network.

Besides the directed network of reported support links, also an *undirected* network can be constructed by defining a link between two households as existing if at least one of the two households mentions the other household as a source of support.

Table 2.3: Network Characteristics for the Support Network in Maramig

	Density	Clustering	Average Pathlength	Prop. reciprocated
Directed Link	0.057	0.175	4.1	0.302
Undirected Link	0.101	0.296	2.6	-

Reported support links within the village.
Based on the report of the 65 surveyed households.

Table 2.3 describes general characteristics of the support network in Maramig, both for the directed network and for the undirected network. For the directed network, of the 4160 $(65 \cdot 64)$ possible links 5.7% are identified as support links (network density), of these 30.2% are reciprocated. In case of the undirected network, 10.1% of the 2080 $(\frac{65 \cdot 64}{2})$ possible links are named. Two additional pieces of information are useful descriptives of the network structure: the average pathlength, which measures the average number of links between any two households in the network, and average clustering, which is a measure for the cohesiveness of a network (Jackson 2008).[14] The average path length of the undirected network is 2.6 and the clustering is 0.296. These measures indicate a tight knit support network. The measures are surprisingly similar to the characteristics of rural social networks in villages in Malawi, Uganda and India, where comparable network data have been collected (for an overview see Chandrasekhar (2016)).

2.5 Empirical Analysis

Two hypotheses guide the empirical analysis. First, it is predicted that an agent only engages in a mutual support arrangement, if she faces a positive probability to be in need of support; that is her alternative resources to cope with an emergency situation alone are insufficient (Hypothesis 1). Second, two agents are predicted to be less likely engaged in a mutual support arrangement the larger the difference in their respective probability to become needy (Hypothesis 2). In addition to these two hypotheses, I investigate whether reciprocated support links need to be analyzed separately from unreciprocated support links, or whether they describe similar types of arrangements.

In the following, I first discuss the empirical specification (Section 2.5.1), in particular the specification of the two main variables included in the model, namely the support arrangement (Section 2.5.1.1) and the probability of neediness (Section 2.5.1.2). I then turn to the dyadic

[14] A household i's clustering coefficient is calculated as the proportion of i's support links for which the alter household is linked to at least one other alter household of i.

analysis (Section 2.5.2), presenting the estimation results of three different models (Sections 2.5.2.1 - 2.5.2.3) that differ in their specification of the support arrangement. At the end, I address potential shortcomings of the main empirical specification (Section 2.5.3).

2.5.1 Specification

In the empirical analysis, the likelihood of a support arrangement between two households i and j is set in relation to the households' respective probability of neediness. The empirical specification thus relies on two main pieces of information: first, information on the *support arrangement* between the households, and second, household characteristics that proxy a household's *probability to become needy*. In the following I discuss the specification of each.

2.5.1.1 Support Arrangement

The specification of a support arrangement is based on the definition presented in Section 2.3.2.2. A support arrangement of a household pair (ij) is defined as existing if i and j have agreed that one household supports the other in the event of an emergency situation; in the case of a one-sided support arrangement, support is expected from one side only; a mutual support arrangement, on the other hand, relies on direct reciprocity: j is expected to support i in case of an emergency and i is expected to support j. Furthermore, the empirical model relies on the assumption that the arrangement is based on a bilateral agreement between i and j; that is, both households are aware of the arrangement and have given their consent.

In the data, a suitable description of inter-household support arrangements is provided by the reported hypothetical sources of support in case of a health emergency, described in Section 2.4.3. This information is used to proxy the unobserved network of support arrangements ς. A reported support link directed from household i to household j is described by s_{ij}, where $s_{ij} = 1$ if household i names household j as a source of support in the event of a health shock, and zero otherwise; and $s_{ji} = 1$ if household j names household i as a source of support in the event of a health shock, and zero otherwise.

This variable has a number of advantages. First, as the variable is defined on a hypothetical situation, the number of observations is not limited to an actual shock event. Second, the arrangement is elicited for the case of a health shock, a type of shock which is, by and large, assumed to be random (i.e., controlling for household size and age structure, in general each household should face a similar probability of a health shock) and idiosyncratic (i.e., within a village, there should be no correlation between households' health shocks within a short period of time); health shocks are thus suitable for inter-household support arrangements within a village. Both assumptions will be verified below. Third, given how the variable was derived, the arrangement can be interpreted as a bilateral agreement: the question was explicitly phrased to consider the willingness of provider and recipient ('*Who would help you...?*', '*Who would you ask...?*'); therefore it seems reasonable to assume that a reported arrangement is based on mutual consent.

While the variable is in many aspects ideal for the empirical analysis, there are two potential

limitations that need to be considered. One concern is that s might underestimate the true network of support arrangements. In case support arrangements are underreported, true mutual support arrangements might be misspecified as one-sided arrangements or non-existing, and true one-sided support arrangements might be misspecified as non-existing. As long as underreporting is random and neither correlated with the explanatory variables included in the estimation nor dependent on the type of existing arrangement, the estimators should be consistent and unbiased.[15] In the main analysis, I include respondent characteristics as additional control variables to address the potential response bias.

Another, related concern is that, while a support link is defined as a link between households, it is reported by individuals.[16] There are three potential issues: First, the individual reporting the links might not be aware of all support links that her household maintains. This concern is less severe for the case of Maramig, as the question on support arrangements had been explicitly phrased for the whole household and typically the head of household was surveyed, who should be aware of most of her household's concerns (particularly so as household size is small). Respondents might still differ in their ability to name the relevant support links; which underlines the importance of including respondent characteristic in the empirical analysis. Second, while the question asks for other *households* as potential source of support, the respondent likely has a particular *individual* in mind and not another household in its entirety. Households with more (adult) members might then be more likely to be named as sources of support simply because there are more individuals in the household. Indeed, this is the case as we will see below (and possibly explains similar findings in Schechter and Yuskavage (2012)). Finally, a support arrangement as defined in Section 2.3.2.2 allows for only one type of support arrangement between two households. However, if the formation of support arrangements is not the decision of a household as a whole, but of the different household members, who have their own budget, then indeed there might be several support arrangements that connect two households; and, even more problematic for the interpretation of the results, two one-sided support arrangements might be falsely interpreted as a mutual support arrangement. This potential case will be adressed when the results are analyzed in more detail (Section 2.5.3.1).

2.5.1.2 Probability to become Needy

In a next step, I determine the explanatory variables to be included in the empirical estimation by analyzing which household characteristics are associated with a higher probability of neediness. For the derivation of these characteristics I revert to the larger data set.

The probability that a household i becomes needy is defined as the probability that i turns to another household from within the village to ask for support in the event of an emergency situation – i.e., the probability that one of i's support arrangements is activated. As support

[15] Misreporting due to overreporting seems less likely. It is not clear why a household i should falsely name household j as a potential source of support. Reporting names was time consuming and required some effort; furthermore, the interviewers were strangers to the villagers and unfamiliar with the village context, making a interviewer demand effect less likely.

[16] Note that eliciting the network on the household level is a common procedure (e.g. see Fafchamps and Gubert (2007b) and Schechter and Yuskavage (2012)) but the implications are typically not discussed.

links were elicited for the case of health emergencies, in our context neediness is conditioned on the event of a health shock. If health shocks are random, then neediness should depend primarily on the alternative resources i has available to cope with a health shock. (Note that neediness also depends on the willingness to deploy alternative resources first before turning to another household, which depends on the costs of deploying the resources as well as on personal preferences; below, I provide some suggestive evidence that in our context households indeed tend to use alternative resources first.)

In order to determine which household characteristics can be generally associated with a higher probability of neediness, I analyze the determinants of asking for support in case of a health shock using the full data set including the 21 other villages that have been surveyed and for which the data are available in unconstrained form. Of the Maramig data, only a random sample of 14 households is included (instead of all households) in order to balance the data with the data of the other villages and to circumvent potential reverse causality effects on the latter analysis.[17] The data set then contains a total of 306 household observations from 22 villages.

In the survey, respondents were asked whether they had experienced a health shock in the past three years and the exact date. Of the 306 households, 48% indicate having experienced a health shock. When the event of a health shock is regressed on household characteristics that could be associated with adverse health related events (namely household wealth, whether the household head completed high school, household size, age distribution of the household members, major sources of income, as well as village fixed effects), only the number of household members that are under the age of 6 is significantly correlated with the event of a health shock (results are reported in Table A.13 in Appendix A.2.1).[18] Village fixed effects are insignificant overall; only in one village are slightly more health shocks reported than in the other villages; yet, they are spread over the three years and do not accumulate within one time period. These results confirm the assumption that the experience of a health shock can be generally viewed as a random event (once household size and demographics are controlled for), and, furthermore, that the majority of health shocks are indeed of idiosyncratic nature.[19]

Respondents, who indicated having experienced a health shock, were asked to identify up to three main strategies they had employed to deal with the shock. In addition, all respondents were asked to identify up to three main strategies they would apply to deal with health emergencies if they faced such a situation today. Both for the case of an actual health shock and for the case of a hypothetical health shock, approximately two thirds of the reported strategies involve the support of neighbors, friends or relatives, the majority of which coming from within the same village (for an overview on all listed strategies, see Tables A.14 and A.15 in Appendix A.2.1). This information allows me to investigate what determines the probability that a coping strategy

[17] More specifically, I include those 14 households that have been surveyed on the first day of the survey period in Maramig. Like in the other villages, these 14 households had been randomly selected from the village's household list.

[18] This effect can be due to illnesses of small children, but is likely also driven by complications during child births; deliveries still bear considerable health risks for the mothers as villages are remote and health centers often difficult to reach.

[19] Various other variables were tested; e.g. income instead of asset wealth, other educational variables as well as respondent characteristics to test for potential response bias. None were significant.

involves the support of other households within the village. The analysis is conducted on the level of the coping strategy. More specifically, I estimate

$$c_{k,i,v} = X_i'\beta + order_k'\alpha + \xi_v + u_{k,i,v} \tag{2.5}$$

where $c_{k,i,v} = 1$ if the strategy k listed by household i living in village v involves support from within the village (in the form of money or in-kind), and 0 otherwise. The analysis is conducted both for the case that an actual health shock has taken place (restricting the data set to those households that have experienced a health shock) as well as for the case of a hypothetical health shock. In line with the framework outlined in Section 2.3.1, the probability of neediness is expected to depend on the alternative resources available to a household, which could include personal wealth, access to financial products such as credit, savings and insurance, as well as access to support outside the village. Therefore, as potential determinants, X_i includes asset wealth, whether the household is covered by health insurance and access to credit, which is defined as given if at least one household member is a member of a bank (commercial bank or microfinance institution), is covered by one of the social security schemes or is a member of a cooperative. Furthermore, I include the number of close household links outside the village elicited in the network section of the questionnaire, as well as a dummy variable for whether the household receives remittances. In addition, I control for the gender and age of the household head, whether the household head has completed high school, household size, and village fixed effects. Summary statistics for all household characteristics included in the analysis are provided in Table A.2 in Appendix A.1.1. Besides the household characteristics, two dummy variables are included, indicating whether a strategy was named as the second or third strategy (*Strategy order* k). This will provide an indication for whether indeed households turn to alternative resources first before asking for support from within the village. The estimation of Specification 2.5 is conducted by logit regression with standard errors clustered on the village level.

Before turning to the results, two comments need to be made. First, one needs to be cautious not to interpret the results as causal. Indeed, there might be considerable endogeneity: households that expect less support from their neighbors might be more inclined to invest in alternative risk management tools, e.g. by acquiring insurance or fostering contacts outside the village; results should thus be interpreted as correlations and not as causal effects. Second, for the analysis of strategies applied in the past, ideally information from the past (i.e., at the time of the health shock) would be included. The data allow for a derivation of values for bank and cooperative membership, access to insurance and coverage through social security systems for the time a health shock occurred, however for the other variables I cannot infer past values. This is problematic for asset wealth, the number of links outside the village, and whether the household receives remittances, as these variables might have changed over the past years and might even be endogenous to the strategy applied at the time of the shock. In a second estimation, I therefore replace these three variables with an index of durable assets, and the number of related households outside the village, excluding former household members. (For details on the derivation and validity analysis of the durable asset index see Appendix A.1.2.)

Table 2.4: Determinants of Neediness (all villages)

	(1)	(2)	(3)
	Needy in past	Needy in past	Needy hypothetically
Age of head	1.007	0.996	1.004
	(0.008)	(0.008)	(0.005)
Female head	0.987	0.898	0.830
	(0.259)	(0.226)	(0.151)
Head completed high school	1.467	1.539	0.811
	(0.511)	(0.512)	(0.159)
Household size	0.794***	0.793***	0.872***
	(0.058)	(0.059)	(0.038)
Asset wealth	0.033***		0.330**
	(0.026)		(0.173)
No. links outside village	0.714***		0.793***
	(0.064)		(0.044)
Remittances recipient	0.377***		0.565***
	(0.115)		(0.100)
Durable asset wealth		0.031***	
		(0.025)	
No. of family hh outside village		0.929***	
		(0.024)	
Access to credit(*)	1.229	1.421	1.410**
	(0.383)	(0.390)	(0.238)
Health insurance(*)	0.468***	0.517**	0.818
	(0.125)	(0.142)	(0.133)
Strategy order 2	1.728*	1.591	2.561***
	(0.540)	(0.463)	(0.484)
Strategy order 3	1.654	1.562	2.504***
	(0.585)	(0.514)	(0.530)
Constant	3.757*	3.825*	1.335
	(2.878)	(3.091)	(0.718)
Village fixed effects	Yes	Yes	Yes
Observations	380	380	896
Mean of dependent variable	0.324	0.324	0.333
χ^2	98.005	83.519	128.991
r_p^2	0.206	0.148	0.125

Logit estimation. Estimators reported as odds ratios. Standard errors in parantheses, clustered at village level.
Columns 1 and 2: determinants of past neediness for households who experienced a health shock.
Column 3: determinants of hypothetical neediness for all households.
(*) In Columns 1 and 2 values for the year of the health shock, in Column 3 values at time of the survey.

The results are reported in Table 2.4. Columns 1 and 2 report the results for strategies applied in the past, Column 3 for hypothetical strategies. Estimators are presented as odds ratios, thus they need to be interpreted as the effect of a one unit change in the independent variable on the probability of neediness. The number of household members as well as asset wealth are significantly negatively correlated with neediness; thus, wealthier households as well as households with more people are less likely to ask for support within their village. Furthermore, households that are well connected outside their village (or have more family outside the village) and households that receive remittances are less likely to ask for support within the village, in case of both past and hypothetical strategies. Also, health insurance coverage is negatively correlated with neediness, yet only significantly so for shocks in the past. Surprisingly, access to credit is positively correlated with neediness, significantly in the case of hypothetical shocks: a strategy named by a household with access to credit is 1.4 times more likely to be 'support from within the village' than another form of risk coping. Finally, seeking support from within the village is more likely to be named as a second or third strategy than as a first strategy; the coefficients are jointly significant. This last result provides some confirmation for the theoretical assumption that people employ their personal resources first before asking for support within their village.

The results indicate that indeed households with access to alternative resources are less likely to be in need of support from within the village. In particular, more wealth, a larger number of connections outside the village and more household members (who could potentially work more to cope with the consequences of a shock) are negatively correlated with being in need of support from within the village. The role of access to credit is surprising. When splitting the variable in the different types of credit access, we can observe that bank membership and social security coverage are driving the results. One potential explanation is that households with credit access have more possibilities to draw on village level support, potentially because they are more trusted. While households with access to credit report on average slightly less links within their village than households without access to credit, it might still be the case that these households are on average more confident that they can seek support from their network than households without access to credit. The data on hand do not permit investigating the relationship in more detail. Nevertheless, this observation points towards a limitation of the theoretical framework: in the framework, personal household level resources are viewed as a substitute for intra-village support, while indeed a higher level of resources could enhance support options from within the village if used as signal of trustworthiness.

To summarize, asset wealth, number of connections outside the village and the size of the household are shown to determine the probability of neediness in case of a health shock and should thus be included as predictors of neediness in the main analysis.

2.5.2 Dyadic Analysis

I now turn to the main analysis of the study and analyze the support arrangement network in Maramig. The likelihood that a mutual support arrangement ς_{ij} between a household i and a

household j is formed is described as

$$P(\varsigma_{ij} = 1) = P(\alpha + X'_{ij}\beta + \epsilon_{ij} \geq 0) \tag{2.6}$$

where X_{ij} includes characteristics of i and j that proxy their respective probability to become needy, and ϵ_{ij} is the link specific error term.

For the characteristics to be included in X, I draw on the findings from Section 2.5.1.2. The following variables are included as proxies for a household's probability to become needy: *Asset wealth*, *Household size*, the proportion of household members between 16 and 59 (*% hh members (16-59)*), as well as the number of related households outside the village (*No. of family hh outside village*), which is used instead of the number of links outside the village due to reverse causality concerns (e.g. a household might invest more in outside connections if there are little support possibilities within the village). As discussed in Section 2.3.2, the size of a household's support network within the village likely affects the probability that one of the support links is activated and should thus be accounted for in the analysis; as the number of support links is inherently endogenous to the model, instead, the number of related households within the village (*No. of family hh within village*) is included as a proxy.[20] Finally, as additional household level controls, age and gender of the household head and a dummy indicating whether the household head completed high school are included (*Age of head*, *Female head*, *Education of head*). Note that as in 90% of the time the household head was also the respondent to the survey, these characteristics overlap considerably with the respondents' characteristics. All of the following estimations have also been conducted including the respondents' characteristics, the coefficients of interest do not change significantly. Potentially endogenous variables, such as access to remittances, coverage by health insurance and access to credit, are excluded for now. I come back to the analysis of these variables further below. Dyadic summary statistics including the sum and the absolute difference of all variables are reported in Table A.3 in Appendix A.1.1.

The dependent variable, ς_{ij}, is based on the reported source of support in case of health emergency, as outlined in Section 2.5.1.1. There are different possibilities to specify this variable, which affect both the specification of the estimation model and the results. In previous studies, different specifications have been used. For the sake of comparison, I conduct three different types of estimations.

First, I analyze the likelihood that household i reports household j as a source of support (Section 2.5.2.1). This specification follows the analysis by Fafchamps and Gubert (2007b). It does not yet inform us about the characteristics of mutual support arrangements but serves as an informative baseline.

Second, I investigate the likelihood of a mutual support arrangement assuming that each reported support link indicates a mutual support arrangement (Section 2.5.2.2). This is a common approach used in the literature (e.g. see De Weerdt (2004)), but it ignores potential one-sided support arrangements.

[20] Note that the questions on the number of related households within and outside the village are part of the socioeconomic questionnaire, they are not derived from the elicited network.

Finally, I turn to the main specification analyzing the likelihood of a support arrangement, distinguishing one-sided from mutual support arrangements by accounting for whether or not a reported support link is reciprocated (Section 2.5.2.3). This approach builds on Schechter and Yuskavage (2012).

2.5.2.1 Likelihood of Support Link

As a baseline, I analyze how i's and j's respective probability to become needy, proxied by their alternative resources, affect the likelihood that i names j as a potential source of support (i.e., $s_{ij} = 1$), disregarding whether or not a support link is reciprocated.

$$s_{ij} = \alpha + \beta_1 X_i + \beta_2 X_j + \delta |X_i - X_j| + \epsilon_{ij} \qquad \text{for } i, j \in N = (1, ..., 65),\ i \neq j. \tag{2.7}$$

The analysis is conducted on the directed network, that is for each pair (i, j), with $i, j \in \mathcal{N} = 1, ..., 65$ and $(i \neq j)$; i.e., 4160 observations. X contains the above described proxies for a household's probability of becoming needy. Note, that the variables are defined such that a higher value in X is associated with a lower probability of neediness. Level and difference effects of each variable are accounted for: β_1 measures the effect of i's predicted neediness on the likelihood of naming other households within the village as a source of support. β_2 measures the effect of j's predicted neediness on the likelihood of being named as a source of support. δ measures the effect of the absolute difference in i's and j's predicted neediness on their likelihood to have a support link.

Specification 2.7 is estimated by a logit model. There are two estimations. First, household characteristics are included as lined out above, in this case standard errors are corrected to account for the correlation across observations (dyadic robust standard errors).[21] Second, instead of the household characteristics X_i and X_j, fixed effects for i and j (i.e., for the ego and for the alter household) are included as well as the absolute differences in the household characteristics. Using fixed effects controls for unobserved household characteristics that affect link formation, yet, in this case, only the impact of the differences in alternative resources, and not the level effects can be analyzed. When fixed effects are included, households that do not report any support links within the village as well as households that are not named as potential sources of support drop out of the analysis (in total 13 households).

Results are reported in Table 2.5. Estimated coefficients for the household head characteristics are excluded from this and the following tables (for the complete tables for the following estimations see Tables A.16 - A.18 in Appendix A.2.2). The results suggest that wealthier households as well as households of larger size are more likely to be named as a potential source of support; however, differences in wealth should not be too large: the larger the difference, the less likely a support link between two households exists. Households with a larger family network within but less family outside the village name on average more support links. Furthermore, a support link is more likely between households with a similarly sized family network. When

[21] I thank Marcel Fafchamps for the provision of the programming code for the estimation of the corrected standard errors.

Table 2.5: Likelihood of Support Link

	(1) $P(s_{ij}=1)$	(2) $P(s_{ij}=1)$
Asset wealth	-0.715	
	(0.671)	
Household size	0.047	
	(0.057)	
% hh members (16-59)	0.104	
	(0.231)	
No. of family hh within village	0.031**	
	(0.013)	
No. of family hh outside village	-0.054*	
	(0.030)	
Alter: Asset wealth	1.790***	
	(0.685)	
Alter: Household size	0.129**	
	(0.062)	
Alter: % hh members (16-59)	0.046	
	(0.334)	
Alter: No. of family hh within village	0.000	
	(0.014)	
Alter: No. of family hh outside village	-0.056*	
	(0.032)	
AbsDiff in Asset wealth	-1.501***	-2.508***
	(0.482)	(0.563)
AbsDiff in Household size	0.060	0.153**
	(0.049)	(0.068)
AbsDiff in % hh members (16-59)	0.181	0.011
	(0.272)	(0.354)
AbsDiff in No. of family hh within village	-0.027*	-0.033**
	(0.015)	(0.016)
AbsDiff in No. of family hh outside village	0.003	-0.062
	(0.030)	(0.039)
Constant	-2.473***	-0.511
	(0.673)	(0.764)
Ego fixed effects	No	Yes
Alter fixed effects	No	Yes
Observations	4160	3357
Control variables	Yes	Yes
Mean of Dependent Variable	0.057	0.070
log likelihood	-859.128	-758.231
χ^2	225.427	191.690
p	0.000	0.000

Logit Estimation on the directed network. Dyadic robust standard errors in parentheses.

Control for the level of and the absolute difference in Age of head, Head completed high school, Female head.

household fixed effects are included (Column 2), the negative effect of differences in wealth remains, and becomes stronger;[22] the difference in household size becomes significantly positively correlated with the existence of a support link, indicating that a support link is typically formed between a smaller and a larger household.

However, while Specification 2.7 uses the complete information of all reported support links, the results cannot be interpreted in the context of mutual support arrangements. The analysis is conducted on the directed network where each link is counted separately and the symmetry requirements of mutual support arrangements, lined out in Section 2.3.2.2, are only fulfilled when fixed effects are included. This limitation is addressed by the following second estimation approach.

2.5.2.2 Likelihood of Mutual Support Arrangement – Naïve Approach

In order to account for the symmetry requirements, the network of support arrangements is analyzed under the assumption that all reported support links are representing mutual support arrangements and that unreciprocated support links are due to underreporting. This approach follows Specification 2.2 described in Section 2.3.2.1.

$$\begin{aligned} \varsigma_{ij} &= \tilde{\alpha} + \tilde{\beta}X_i + \tilde{\beta}X_j + \tilde{\delta}|X_i - X_j| \\ \text{where } \varsigma_{ij} &= 1 \textit{ if } s_{ij} = 1 \textit{ or } s_{ji} = 1 \\ \varsigma_{ij} &= 0 \textit{ if } s_{ij} = 0 \textit{ and } s_{ji} = 0 \qquad \text{for } i, j \in N = (1, ..., 65),\ i < j. \end{aligned} \tag{2.8}$$

The analysis is conducted on the undirected network graph ($i, j \in \mathcal{N} = 1, ..., 65$ and ($i < j$), i.e., 2080 observations). Regressors are included to maintain the symmetric structure – i.e., the sum, $(X_i + X_j)$, to measure level effects, and the absolute difference, $|X_i - X_j|$.[23] Estimation is done by logit regression; in a first estimation, standard errors are corrected to account for the correlation across observations, in the second estimation household fixed effects are included. $\tilde{\beta}$ measures the effect of the combined level of i's and j's predicted neediness on their likelihood to form a mutual support arrangement, while $\tilde{\delta}$ measures the effect of the absolute difference of i's and j's expected neediness. A higher X is associated with a lower likelihood of neediness.

In line with Hypothesis 1, $\tilde{\beta}$ is expected to be smaller than zero; the more resources i and j have available, and thus the smaller i's and j's predicted neediness, the lower the likelihood that they engage in a mutual support arrangement. In line with Hypothesis (2), $\tilde{\delta}$ is expected to be smaller than zero; the larger the differences in i's and j's predicted neediness, the less likely it is that they form a mutual support arrangement.

Results are reported in Table 2.6; as in the previous table, Column 2 reports the results of the fixed effects regression. The estimated coefficients differ considerably from the estimation of

[22] The change is to some extent driven by those ten households that do not name any support links within the village and drop out of the analysis when fixed effects are included; these households are overall wealthier than the other households.

[23] Note that $\tilde{\beta}(X_i + X_j)$ is split in $\tilde{\beta}X_i + \tilde{\beta}X_j$, for presentation reasons only.

Table 2.6: Likelihood of Mutual Support Arrangement – Naïve Approach

	(1) $P(\varsigma_{ij}=1)$	(2) $P(\varsigma_{ij}=1)$
Asset wealth	0.595*	
	(0.330)	
Household size	0.098***	
	(0.038)	
% hh members (16-59)	0.107	
	(0.191)	
No. of family hh within village	0.012	
	(0.009)	
No. of family hh outside village	-0.037	
	(0.023)	
Alter: Asset wealth	0.595*	
	(0.330)	
Alter: Household size	0.098***	
	(0.038)	
Alter: % hh members (16-59)	0.107	
	(0.191)	
Alter: No. of family hh within village	0.012	
	(0.009)	
Alter: No. of family hh outside village	-0.037	
	(0.023)	
AbsDiff in Asset wealth	-0.523	-1.313**
	(0.511)	(0.579)
AbsDiff in Household size	0.108**	0.231***
	(0.047)	(0.077)
AbsDiff in % hh members (16-59)	0.090	-0.431
	(0.309)	(0.406)
AbsDiff in No. of family hh within village	-0.018	-0.020
	(0.017)	(0.018)
AbsDiff in No. of family hh outside village	-0.004	-0.059
	(0.031)	(0.045)
Constant	-1.860***	-2.492***
	(0.700)	(0.752)
Ego fixed effects	No	Yes
Alter fixed effects	No	Yes
Observations	2080	1854
Control variables	Yes	Yes
Mean of Dependent Variable	0.055	0.062
log likelihood	-647.404	-568.690
χ^2	69.712	151.925
p	0.000	0.030

Logit Estimation on the undirected network. $\varsigma_{ij}=1$ if $s_{ij}=1$ or $s_{ji}=1$.

Dyadic robust standard errors in parentheses.

Control for the level of and the absolute difference in Age of head, Head completed high school, Female head.

Specification 2.7. The combined level effects of households' asset wealth as well as of household size is significant and positive. Wealthier households and larger households seem *more* likely to form mutual support arrangements than poorer households or households of smaller size. This is contrary to the prediction of Hypothesis 1, as asset wealth and household size have been shown to be associated with a *lower* probability of neediness. When including household fixed effects the effects are similar as the fixed effect estimation of Specification 2.7, households are more likely to form support arrangements if they have a similar level of wealth and if household size differs. This is not particularly surprising, Specification 2.7 with fixed effects is similar to Specification 2.8 with fixed effects, with the exception that the estimation of the latter is conducted on a subset of the support links analyzed in the former.

In summary, if indeed every support link indicates a mutual support arrangement, then, based on these results, Hypothesis 1 would be rejected. While small household size and low wealth have been shown to be strong predictors of neediness, these variables do not affect the formation of mutual support arrangements as expected; indeed households with more alternative resources in terms of household members and wealth, are predicted to be *more* likely to engage in mutual support arrangements. With regard to Hypothesis 2, the interpretation is not as straightforward. Households with a larger difference in wealth indeed seem to have a lower likelihood to form a mutual support arrangement, yet the effect is only significant when fixed effects are included. The differences in the other variables of interest (i.e., resources within the household, within the village or outside the village) have either no significant effect or the effect is opposite to our expectation (as for the case of household size).

However, the definition of mutual support arrangements used in Specification 2.8 might be inadequate. As discussed in Section 2.3.2.2, the definition potentially combines mutual with purely one-side support arrangements, which do not follow the motive of risk-sharing. Hence, it might be sensible to differentiate between support links that are reciprocated from those that are unreciprocated. This is the next step.

2.5.2.3 Likelihood of Mutual Support Arrangement – Accounting for Reciprocation

The third estimation approach follows Model 2.4 described in Section 2.3.2.2 and accounts for the direction of a support link and for whether or not a support link is reciprocated. The model builds on the approach by Schechter and Yuskavage (2012).[24] I estimate the joint likelihood using a multinomial logit specified as follows:

[24]Note, however, that Schechter and Yuskavage (2012) do not address the issue of bilateral and unilateral link formation. They only study the reports from one side, neglecting the report from the other party (which furthermore might result in double counting some of the arrangements).

$$
\begin{aligned}
P(\dot{\varsigma}_{ij} = 1) &= e^{\alpha'' + \beta_1'' X_i + \beta_2'' X_j + \delta_1'' |X_i - X_j|} k^{-1} \\
P(\dot{\varsigma}_{ij} = 2) &= e^{\alpha'' + \beta_2'' X_i + \beta_1'' X_j + \delta_1'' |X_i - X_j|} k^{-1} \\
P(\dot{\varsigma}_{ij} = 3) &= e^{\alpha'' + \beta_3'' X_i + \beta_3'' X_j + \delta_3'' |X_i - X_j|} k^{-1} \\
k = 1 + e^{\alpha'' + \beta_1'' X_i + \beta_2'' X_j + \delta_1'' |X_i - X_j|} &+ e^{\alpha'' + \beta_2'' X_i + \beta_1'' X_j + \delta_1'' |X_i - X_j|} + e^{\alpha'' + \beta_3'' X_i + \beta_3'' X_j + \delta_3'' |X_i - X_j|} \\
\text{where } \dot{\varsigma}_{ij} &= 1 \textit{ if } s_{ij} = 1 \textit{ and } s_{ji} = 0 \\
\dot{\varsigma}_{ij} &= 2 \textit{ if } s_{ij} = 0 \textit{ and } s_{ji} = 1 \\
\dot{\varsigma}_{ij} &= 3 \textit{ if } s_{ij} = 1 \textit{ and } s_{ji} = 1 \\
\dot{\varsigma}_{ij} &= 0 \textit{ if } s_{ij} = 0 \textit{ and } s_{ji} = 0 \qquad \text{for } i, j \in N = (1, ..., 65),\ i < j.
\end{aligned}
\tag{2.9}
$$

Again the analysis is conducted on the undirected graph, that is for each pair (i, j), with $i, j \in \mathcal{N} = 1, ..., 65$ and $(i < j)$; i.e., 2080 observations. For the estimation I use the reference category of $\dot{\varsigma}_{ij} = 0$. The parameters are constrained to account for symmetry. In particular, the estimation of $P(\dot{\varsigma}_{ij} = 2)$ mirrors the estimation of $P(\dot{\varsigma}_{ij} = 1)$: the coefficients of the receivers' characteristics in one-sided support – i.e., the ego household in arrangement $\dot{\varsigma}_{ij} = 1$ and the alter household in $\dot{\varsigma}_{ij} = 2$ – are constrained to be equal, and the same holds for the providers' characteristics; the intercept as well as the coefficients of the absolute differences in characteristics are constrained to be the same in $\dot{\varsigma}_{ij} = 1$ and $\dot{\varsigma}_{ij} = 2$. Furthermore, in the estimation of $P(\dot{\varsigma}_{ij} = 3)$ – i.e., the mutual support arrangement – level effects are constrained to be the same for the characteristics of i and the characteristics of j (similar to Specification 2.8).

$\dot{\varsigma}_{ij} = 3$ is assumed to describe mutual support arrangements. I expect that these arrangements are more likely if the predicted neediness of both parties is sufficiently high ($\beta_3'' > 0$), yet the difference in their predicted neediness is small ($\delta_3'' < 0$). Furthermore, I investigate the difference in reciprocated versus unreciprocated support links. If size and direction of the coefficients of interest differ considerably for reciprocated support links ($\dot{\varsigma}_{ij} = 3$) compared to unreciprocated support links ($\dot{\varsigma}_{ij} = 1$ and $\dot{\varsigma}_{ij} = 2$), then this is an indication that unreciprocated support links follow different mechanisms than reciprocated support links. The multinomial logit model is based on the assumption of independence of irrelevant alternatives (IIA). That is, the proportion of probabilities between two alternatives should be independent on the existence of a third alternative (Greene 2012). This assumption can be tested formally using the Hausman-McFadden Test (Hausman and McFadden 1984). In particular, I test whether the coefficients for one-sided suport arrangements change significantly when the option of mutual support arrangements is excluded. This hypothesis can be rejected.[25]

[25]Comparing the two models leads to a test statistics of $\chi^2(25) = -0.47$. A negative χ^2 is generally viewed as support for the hypothesis that there is no difference (Vijverberg 2011). An alternative approach is to include the covariance when estimating the variance of the estimates' differences (Weesie 1999). This ensures that testing results are always well defined; furthermore, it allows for clustering standard errors, which is not feasible with the conventional Hausman-McFadden Test, and for testing the models separately. This test supports the results for both types of one-sided support arrangements ($\chi^2(25) = 18.80$, $p = 0.81$).

Results are reported in Table 2.7; in Column 1, results are reported for one-sided support arrangements – where i (the ego household) names j (the alter household) as a source of support, but j does not name i ($\dot{\varsigma}_{ij} = 1$); in Column 2, results are reported for a one-sided support arrangement – where j names i as a source of support but i does not name j ($\dot{\varsigma}_{ij} = 2$); in Column 3, results are reported for mutual support arrangements – i.e., i names j and j names i ($\dot{\varsigma}_{ij} = 3$). With regard to one-sided arrangements, I find that wealthier households and households of larger size are typically named as providers in one-sided support arrangements. As expected, with more family in the village, the likelihood of being a recipient of support increases, presumably because there are more opportunities to ask for support. The effect of absolute differences in wealth is negative; that is, even though a one-sided support arrangement is more likely when the provider household is wealthy, there should be no large difference in wealth between the household providing support and the household receiving support.

All these characteristics play no role for the formation of mutual support arrangements (Column 3). Mutual support arrangements are more likely when both parties do not have many family connections outside the village, when they have a similar level of wealth and a similarly large family support network. Furthermore, households with more relatives in the village are also more likely to engage in mutual support. Difference in wealth seems to play an especially important role. The effect is much more pronounced than for the case of one-sided arrangements. In particular, if the difference in wealth reduces by one standard deviation (0.17), two households are twice more likely to engage in a mutual support arrangement than to engage in no arrangement ($e^{-4.33 \cdot -0.17} = 2.09$); for the case of one-sided support arrangements the scaling factor is only 1.18 ($e^{-0.95 \cdot -0.17}$).

So far, I have ignored alternative resources which are potentially endogenous to support arrangement formation but have, nevertheless, been shown to explain the probability of neediness (Section 2.5.1.2). When I include these variables (namely, coverage by health insurance, access to credit and receiving remittances) results change slightly (results are reported in Appendix A.2.2 in Table A.20). One-sided support links are more likely if the recipient has access to credit (which is reminiscent of the previous results discussed in Section 2.5.1.2) and if the provider is covered by health insurance; remittances seem to play no role. Contrary to this, mutual support arrangements are less likely between households that receive remittances. However, neither access to credit nor coverage by health insurance are predicted to explain the formation of mutual support arrangements.

Summarizing the results, first, I find that the determinants for reciprocated support links differ considerably from the determinants for unreciprocated support links. The results indicate that indeed unreciprocated support links rather describe one-sided support arrangements that follow a different mechanism. Second, the hypotheses on mutual support arrangements can only partly be confirmed. Hypothesis 1 predicts that households with less alternative resources on hand and thus with a larger probability of neediness are generally more likely to form mutual support arrangements. However, neither wealth, nor household size, nor member composition, which all are found to play an important role in predicting neediness, seem to determine whether

Table 2.7: Likelihood of Mutual Support Arrangement – Accounting for Reciprocation

	(1) $P(\dot{\varsigma}_{ij}=1)$	(2) $P(\dot{\varsigma}_{ij}=2)$	(3) $P(\dot{\varsigma}_{ij}=3)$
Asset wealth	-0.847	1.858***	0.256
	(0.604)	(0.647)	(1.044)
Household size	0.069	0.143***	0.077
	(0.062)	(0.055)	(0.091)
% hh members (16-59)	0.077	-0.010	0.184
	(0.267)	(0.434)	(0.552)
No. of family hh within village	0.025*	-0.011	0.033*
	(0.014)	(0.017)	(0.019)
No. of family hh outside village	-0.020	-0.030	-0.182*
	(0.030)	(0.040)	(0.097)
Alter: Asset wealth	1.858***	-0.847	0.256
	(0.647)	(0.604)	(1.044)
Alter: Household size	0.143***	0.069	0.077
	(0.055)	(0.062)	(0.091)
Alter: % hh members (16-59)	-0.010	0.077	0.184
	(0.434)	(0.267)	(0.552)
Alter: No. of family hh within village	-0.011	0.025*	0.033*
	(0.017)	(0.014)	(0.019)
Alter: No. of family hh outside village	-0.030	-0.020	-0.182*
	(0.040)	(0.030)	(0.097)
AbsDiff in Asset wealth	-0.949*	-0.949*	-4.330***
	(0.525)	(0.525)	(1.200)
AbsDiff in Household size	0.132***	0.132***	-0.136
	(0.045)	(0.045)	(0.158)
AbsDiff in % hh members (16-59)	-0.038	-0.038	0.746
	(0.310)	(0.310)	(0.676)
AbsDiff in No. of family hh within village	-0.014	-0.014	-0.060*
	(0.020)	(0.020)	(0.035)
AbsDiff in No. of family hh outside village	-0.013	-0.013	0.069
	(0.042)	(0.042)	(0.134)
Constant	-2.449***	-2.449***	-4.280*
	(0.785)	(0.785)	(2.369)
Observations	2080		
Control variables	Yes		
log likelihood	-815.942		
χ^2	272.959		
p	0.000		

Multinomial logit estimation. Dyadic robust standard errors in parentheses.

Control for the level of and the absolute differences in Age of head, Education of head, Female head.

a household is part of a mutual support arrangement or not. Only the number of family outside the village and access to remittances is negatively correlated with the formation of mutual support arrangements. Hypothesis 2 predicts that households which differ in their probability to become needy are less likely to form mutual support arrangements. In some aspects this hypothesis is confirmed. Mutual support arrangements seem to be mainly formed between households of a similar wealth level and with a similarly large family network within the village. However, differences in none of the other resources are found to have a significant effect. Assuming the estimation model is correctly specified, these results suggest that the proposed framework described in Section 2.3.1 can explain some of the structure of the support network in Maramig, while other aspects remain unexplained. Part of this might be driven by shortcomings of the empirical specification. This will be addressed in the following section.

2.5.3 Limitations and Extended Analysis

There are a number of potential shortcomings of the estimation approach. First, what I call a mutual support arrangement, might indeed just be two one-sided support arrangements; that is, the report by i and the report by j do not refer to the same arrangement but to two different one-sided arrangements. Second, in the specification above, household characteristics are included as if each had a separate effect on the formation of support arrangements, while indeed these characteristics are supposed to function as a combined predictor of neediness. Third, asset wealth is estimated to have a strong effect on support link formation; however, wealth can be endogenous to the formation of support arrangements: similar levels of wealth might be an outcome rather than a driver of the formation of a support arrangement (as shown theoretically in Bourlès et al. ("Altruism in Networks") and Bramoullé and Kranton (2007)). Fourth, results might not be driven by the households' probability of neediness but by other unobserved characteristics that have been shown in previous studies to play a role in risk-sharing formation processes such as risk and time preferences (Charness and Genicot 2009) or trustworthiness (Attanasio et al. 2012). Fifth, while I analyze mutual support arrangements in a network context, the model does not allow the network structure to affect link specific utilities. In particular, it has been shown that network structure might affect compliance (e.g. in Jackson et al. (2012)). The model above, however, does not account for such interdependencies.

In order to address these concerns, I conduct some additional analyses. I address the first concern, by comparing two models that, respectively, do and do not account for reciprocity, in order to ensure that presumed mutual support arrangements are not indeed two one-sided support arrangements (Section 2.5.3.1). To address the three concerns with regard to the explanatory variables, I develop a 'neediness score' based on characteristics that are exogenous to the formation of a support arrangement; with this composite predictor, which should be unaffected by any unobserved household characteristics or by the support link formation process itself, I re-estimate Specification 2.9 (Section 2.5.3.2). Finally, I analyze how results differ when a measure for the local network structure is included in the estimation (Section 2.5.3.3).

2.5.3.1 Analyzing reciprocity

Could potentially a reciprocated support link represent not a mutual support arrangement but rather two one-sided support arrangements? If this is the case, a model which accounts for reciprocity should not have better explanatory power than a model which does not account for reciprocity. This is analyzed by comparing the estimation results of Specification 2.7, which estimates all support links independently of whether they are reciprocated or not, with a specification, that takes reciprocity into account. In this latter specification, links that are reciprocated ($s_{ij} = 1$ *and* $s_{ji} = 1$) are distinguished from links that are not reciprocated ($s_{ij} = 1$ *and* $s_{ji} = 0$). The estimation is conducted by multinomial logit on the directed network graph (i.e., for each pair (i, j), with $i, j \in \mathcal{N} = 1, ..., 65$ and $(i \neq j)$ – i.e. 4160 observations), with the reference category of ($s_{ij} = 0$ *and* $s_{ji} = 0$). Note that this specification is very similar to Specification 2.9. Likewise, for reciprocated links coefficients are constrained to fulfill symmetry requirements.

$$\begin{aligned} P(s_{ij} = 0 \text{ and } s_{ji} = 1) &= e^{\alpha_1'(X_i)+\alpha_2'(X_j)+\delta_1'|X_i-X_j|}k^{-1} \\ P(s_{ij} = 1 \text{ and } s_{ji} = 1) &= e^{\alpha_3'(X_i)+\alpha_3'(X_j)+\delta_3'|X_i-X_j|}k^{-1} \\ k = 1 + e^{\alpha_1'(X_i)+\alpha_2'(X_j)+\delta_1'|X_i-X_j|} &+ e^{\alpha_3'(X_i)+\alpha_3'(X_j)+\delta_3'|X_i-X_j|} \end{aligned} \tag{2.10}$$

The estimation of Specification 2.7 is compared with the estimation of Specification 2.10. Goodness of fit measures for both analyses are reported in Table 2.8. Yet, the measures cannot be directly compared as the models are not nested. Two aspects can be analyzed. First, I test whether the estimators for the probability to initiate a support link significantly differ when we account for reciprocity, that is we compare the estimators of Specification 2.7 with the estimators for the unreciprocated support links in Specification 2.10. The Wald test indicates that indeed these estimators differ significantly: with a $\chi^2(25) = 110.46$ we can reject the hypothesis that both estimators are similar.

Table 2.8: Model Comparison

	Specification 2.7	Specification 2.10
	(1)	(2)
	Logit	Multinomial Logit
Observations	4160	4160
log likelihood	-859.128	-965.589
χ^2	225.427	413.040
p	0.000	0.000
Mean squared error		
- of unreciprocated links	0.0394	0.0391
	(0.0026)	(0.0027)
- of reciprocated links	0.0148	0.0143
	(0.0019)	(0.0017)

Specification 2.7 does not distinguish reciprocated from unreciprocated links.
Specification 2.10 distinguishes reciprocated from unreciprocated links.
Standard errors in parentheses.

Second, we can analyze whether Specification 2.7 performs better in predicting reciprocated and unreciprocated links than Specification 2.10, by calculating the mean squared prediction error for each type of link. For unreciprocated links the mean squared error is 0.0394 for the predictor based on Specification 2.7 and 0.0391 for the predictor for Specification 2.10. While the difference is not large, it is significantly different from zero. For unreciprocated links the mean squared error is 0.0148 for the predictor based on Specification 2.7 and 0.0143 for the predictor for Specification 2.10. Again the difference is significantly different from zero. That is, Specification 2.10 performs overall better in predicting both unreciprocated and reciprocated links than Specification 2.7.

However, the predictive power of both models is very poor. This is mainly due to the 'rare-event' nature of the data and is a well-known limitation to dyadic regression analysis of link formation.

2.5.3.2 Neediness Score

I develop a predictor of neediness that is based on household characteristics that are exogenous to the support arrangement formation process. In particular, exogenous household characteristics are weighted by a factor that is derived by analyzing past neediness using the larger data set of the 22 villages (including a sample of 14 households from Maramig), the same data set which is used in Section 2.5.1.2. Using the larger data set and not limiting the analysis to the data of Maramig circumvents the potential problem of reverse causality; if the predictor would be developed based on the Maramig data set only, the derived weights might be affected by the existing support links.

Based on the derived weights, for each household in Maramig, a score is developed that describes this particular household's probability to become needy. This score is similar to the score derived in propensity score matching techniques used for treatment effect analyses (Greene 2012, pp.934) In particular I estimate

$$Y_{m,v} = \beta X_m + \xi_v + \epsilon_{m,v} \text{ for } m = 1, ..., n \text{ and } v = 1, ..., 22 \tag{2.11}$$

where $Y_{m,v} = 1$ if household m living in village v has been needy in the past (i.e., if m has experienced a health shock and has turned to another household within her village to ask for support) and $Y_{m,v} = 0$ otherwise. X includes household level characteristics that are exogenous to the probability of neediness. In addition, village fixed effects ξ are included. Based on the estimator $\widehat{\beta}$, I can derive for each household i in Maramig a neediness score $\widehat{needy_i}$:

$$\widehat{needy_i} = \hat{\beta} X_i \text{ for } i = 1, ..., 65 \tag{2.12}$$

The following variables are included in X: the education of the household head, the gender and age of the household head, the age distribution of household members and the number of other households within the village the respondent household is related to. Specification 2.11 is estimated via maximum likelihood. Results are reported in Table A.21 in Appendix A.2.3. The

score is standardized to be between 0 and 1. Summary statistics for the derived neediness score for the households of Maramig are described in Table A.22 and the distribution is depicted in Figure A.1 in Appendix A.2.3.

I then reestimate Specification 2.8 using as explanatory variables only the level and the differences in the neediness score of the ego and the alter household, controlling for household head characteristics (see Table A.3 in Appendix A.1.1 for the summary statistics). Results are reported in Table 2.9, Part 1 (for the full table see Table A.19 in Appendix A.2.2). The level of predicted neediness does not affect the likelihood that a household is a recipient or a provider in a support arrangement significantly, neither for one-sided nor for mutual support arrangements. However, differences in neediness play a role. Households are more likely to be part of one-sided support arrangements the larger the difference in their predicted neediness. The sign of the estimated level effects indicate that in a one-sided support arrangement the provider has on average a slightly lower predicted probability of neediness while the recipient has a slightly higher predicted probability of neediness. This is not the case for mutual support arrangements. The more households differ in their predicted neediness, the less likely it is that a mutual support arrangement is formed. In particular, a one standard deviation reduction in the absolute difference in predicted neediness (0.24), increases the likelihood of a mutual support arrangement compared to having no arrangement by around 40% ($e^{-1.37 \cdot -0.24} = 1.39$), while it reduces the likelihood of a one-sided support arrangement by 16% ($e^{0.73 \cdot -0.24} = 0.84$). Interestingly, it does not seem to be the case that households with a higher probability of neediness are more likely to form a mutual support arrangement; while the coefficients are not significant, the direction of the effect is opposite to what is predicted in Hypothesis 2.

In summary, the findings of the previous analysis are confirmed. Hypothesis 1 needs to be rejected. It seems not to be the case that households with a particularly high probability of neediness are more likely to form mutual support arrangements. There is some support for Hypothesis 2. Indeed, households which form mutual support arrangements typically do not differ strongly in their probability of becoming needy. However, the estimated effects are only marginally significant and explain little of the overall variance. There seem to be other, possibly more important, forces that affect the formation and sustainability of support arrangements. One of these potential forces is discussed next.

2.5.3.3 Including Network Characteristics

An important piece of information, which was disregarded until now, is the structure of the local support network. The local support network can affect compliance. Theoretically, Jackson et al. (2012) show that the participation constraint discussed in Section 2.3 can be ameliorated in a network setting, when in case i defects on j, i does not only jeopardize the arrangement with j but also arrangements with all those partners that she has in common with j. In particular, they show that in case $c > \frac{\delta\left(p_i\, v - p_j\, c\right)}{1-\delta}$, a mutual support arrangement network ς can still be sustained if all links in ς are 'backed,' where a link between i and j is defined as 'backed' if there

Table 2.9: Likelihood of Mutual Support Arrangement - Neediness Score

	Part (1) Mutual Support			Part (2) Mutual Support		
	$P(\dot{\varsigma}_{ij}=1)$	$P(\dot{\varsigma}_{ij}=2)$	$P(\dot{\varsigma}_{ij}=3)$	$P(\dot{\varsigma}_{ij}=1)$	$P(\dot{\varsigma}_{ij}=2)$	$P(\dot{\varsigma}_{ij}=3)$
$\widehat{needy}$	-0.285	0.287	-0.404	-0.456	0.767	1.506**
	(0.229)	(0.452)	(0.660)	(0.504)	(0.562)	(0.590)
Alter: $\widehat{needy}$	0.287	-0.285	-0.404	0.767	-0.456	1.506**
	(0.452)	(0.229)	(0.660)	(0.562)	(0.504)	(0.590)
AbsDiff in $\widehat{needy}$	0.728*	0.728*	-1.366*	0.153	0.153	-1.238
	(0.428)	(0.428)	(0.780)	(0.651)	(0.651)	(1.206)
commonfriend				1.052**	1.052**	3.990***
				(0.419)	(0.419)	(0.987)
$\widehat{needy}$*commonfriend				0.273	-0.759	-3.006***
				(0.754)	(0.617)	(1.078)
(Alter: $\widehat{needy}$)*commonfriend				-0.759	0.273	-3.006***
				(0.617)	(0.754)	(1.078)
(Absdiff in $\widehat{needy}$)*commonfriend				0.660	0.660	0.320
				(0.882)	(0.882)	(1.877)
Constant	-1.980***	-1.980***	-2.450*	-2.429***	-2.429***	-5.069***
	(0.655)	(0.655)	(1.419)	(0.719)	(0.719)	(1.530)
Observations	2080			2080		
Control variables	Yes			Yes		
log likelihood	-839.892			-799.244		
χ^2	166.661			551.357		
p	0.000			0.000		

Multinomial logit estimation. Dyadic robust standard errors in parentheses.
Control for the level of and the absolute differences in Age of head, Education of head, Female head.

is at least one other agent k who is linked both with i and with j.[26] Along similar lines, we could expect that even if the difference in the probability of becoming needy is large, a mutual support arrangement between two households might still be formed, if there is a third household that is linked to both.

This can be analyzed by including the information of whether two households have a friend in common. This information is derived from the friendship network that has been elicited as part of the network questionnaire. I define a link between two households as backed if they have at least one friend in common. In particular, for each household pair (ij) with $i < j$, $commonfriend_{ij} = 1$ if at least one of the households, which household i lists as 'close households,' is also listed by household j as a 'close household.' I reestimate Specification 2.8 using the neediness score of the ego and the alter household as well as the absolute differences and include the indicator for whether or not a common friend exists, as well as the interaction between this indicator and the neediness score. Note that for reasons of symmetry, the coefficients are constrained in a similar way as above. In particular, the estimation of $P(\dot{\varsigma}_{ij} = 2)$ mirrors the estimation of $P(\varsigma_{ij} = 1)$ and the effect of $commonfriend_{ij}$ is constrained to be the same for $P(\dot{\varsigma}_{ij} = 1)$ and $P(\dot{\varsigma}_{ij} = 2)$.

Results are reported in Table 2.9 Part 2. Generally, arrangements are more likely if a link is backed by a common friend; the effect is particularly strong for the case of mutual support arrangements. Households are fifty times more likely to engage in a mutual support arrangement if they have a common friend ($e^{3.99}$). The exceptionally high effect suggests that there is strong clustering which, however, cannot be well captured with ordinary regression analysis. It might partly also be driven by the small number of observed reciprocated support links in the data; I will discuss this issue below. Once we account for the common friend, the coefficients for the level and absolute differences in predicted neediness change. Mutual support links are now more likely if both households have a high probability of neediness, as predicted with Hypothesis 1. Interestingly, this effect is reduced once a link is backed. More specifically, a one standard deviation increase in predicted neediness increases the likelihood that a household engages in a mutual support arrangement with a household that has no friend in common by 44% ($e^{1.51 \cdot 0.24} = 1.44$); while it reduces the likelihood to engage in a mutual support arrangement with a household that has a friend in common by 31% ($(e)^{0.24 \cdot (1.51 - 3.01)} = 0.69$); the coefficients are jointly significant. However, as the latter effect is very small compared to the large effect of common friendship, a better way to describe the results is the following: while a lower predicted neediness reduces the likelihood that a household engages in mutual support, once two households have a friend in common they might engage in a mutual support arrangement even if their probability of neediness is low. The coefficients of the absolute difference in neediness turn insignificant. Again the interaction with the common friend counteracts the main effect as predicted. One way of interpreting these results is that having a friend in common could be an indicator for a different type (potentially more friendship based) link. With a common friend, other mechanisms than purely 'quid-pro-quo' motives come into play. For example, support might be provided even if it is not necessarily needed, rather as a sort of custom to strengthen the relationship. This analysis shows that the local network structure can have important implications for the formation processes of

[26] Note that in the paper, Jackson et al. (2012) use the word 'supported' instead of 'backed.'

social links, an aspect, which can only insufficiently be captured by dyadic regression analysis.

2.6 Discussion

In this study I analyze the role of households' probability of neediness for the formation of mutual support arrangements within villages. A model that assumes balanced reciprocity as the guiding principle predicts that households engage in mutual support arrangements if they have insufficient alternative resources to cope with a shock in isolation; that is if they face a positive probability of neediness (Hypothesis 1). Furthermore, the model predicts that a mutual support arrangement between two households is less likely to sustain the larger the households differ in their respective probability of neediness (Hypothesis 2).

The predictions are tested using census data of a fishing village in Maramig. The findings are ambiguous. While households that form mutual support arrangements seem to have generally less resources from outside the village and are thus more dependent on support from within the village, they are not necessarily more deprived in terms of wealth, number of working-age household members or access to credit. The first hypothesis can thus not be confirmed. There is some support for Hypothesis 2. Two households are found to be more likely to engage in mutual support arrangements if they have a similar level of wealth and a similarly large family network within the village. Likewise, households with a similar predicted neediness score, which is derived using exogenous household characteristics, are more likely to form a mutual support arrangement. Results change considerably, when an indicator for the local network structure is included as an explanatory variable. Results suggest that a reduction in a household's predicted neediness reduces the likelihood that the household engages in a mutual support arrangement with a household that shares no common friend, while it increases the likelihood to engage in a mutual support arrangement with a household that shares a common friend. The findings suggest that the motive of risk-sharing under the principle of balanced reciprocity, is not entirely well-suited to explain the structure of mutual support arrangements. These findings are in line with De Weerdt (2004) and De Weerdt and Fafchamps (2011). Analyzing actual inter-household transfer data, the authors show that some of the common prediction of risk-sharing models do not hold. Other motives need to be accounted for, such as social preferences which are shown to reduce the incentive to defect in a risk-sharing arrangement (Foster and Rosenzweig 2001; Lin et al. 2014).

As pointed out throughout the chapter, there are a number of important limitations on the analysis that need to be considered. First, assumptions of the underlying theoretical framework might not hold. Second, the data on hand might miss valuable information. Third, the estimation approach cannot sufficiently accommodate the local network structure. I will briefly discuss each.

The theoretical framework makes a number of assumptions. Most importantly, the personal resources of a household are assumed to be fixed. But, indeed, the utilization of these resources is unlikely to be costless; resources are depletable. If support from another household in the village is less costly than the use of personal resources, then also households with sufficient personal resources on hand might still engage in mutual support arrangements. Furthermore, in the model,

higher personal resources are associated with a higher propensity to renege on an agreement, as the costs of living in autarchy are reduced. Thus, if i considers forming an arrangement with j, the level of j's resources should not only signal to i the probability that j will be in need of future support but also the risk j might renege on the agreement when i is in need of support. However, if this risk is only insufficiently incorporated in people's link formation decision (as shown by Lin et al., 2014) or if there are other costs of living in autarchy, such as social costs, which are neglected by the model but reduce the risk of defection, then a household with more personal resources might overall be considered more trustworthy and therefore more attractive as a support arrangement partner. This could explain some of the unexpected findings. In future research, these aspects should be incorporated into the theoretical framework and the resulting predictions tested empirically.

There are several data related limitations. In particular, the data analysis is limited to support arrangements within a village; arrangements with households residing outside the village are neglected, as those households had not been surveyed and thus information on their socio-economic characteristics are missing. However, there is considerable evidence that support networks span across villages (De Weerdt and Dercon 2006; Mazzocco and Saini 2012). Some of the households that have been identified as not engaging in mutual support arrangements might indeed have such arrangements with households in neighboring villages. Furthermore, as only data from one village is used, the sample size is small; in particular, the number of observed mutual support arrangements is small which limits the inferences one can make. Finally, the data do not allow me to verify whether a reported source of support – i.e., the household that would provide the support – is aware of this role. Despite the careful definition of the support link variable, there might still be the possibility that this variable describes a *desire* for an arrangement rather than an actual arrangement which both parties are aware of. This could undermine the estimation approach. Future network surveys should include questions both on the provision as well as on the recipience of support, in order to disentangle actual from desired support arrangements.

Lastly, and possibly of most importance, the network structure cannot be incorporated in dyadic regression analysis. With pairwise regressions, link decisions are assumed to be independent, yet in the case of support arrangements, this is hardly a realistic assumption. For example, the number of support arrangements an agent i maintains with her neighbors, $N_i(\varsigma) = \{j \mid ij \in \varsigma\}$, likely affects the probability that one of the links will be activated – i.e., the probability that neighbor j is asked by i probably reduces with the number of support arrangements i maintains. Thus, the net utility an agent j derives from having a support link with i would depend on N_i, a feature that the estimation approach cannot incorporate. Including the number of family households in the village as a proxy for the size of the support network is unlikely to be a sufficient remedy. Furthermore, I only analyze direct links, yet it has been shown both theoretically ("Altruism in Networks") and empirically (Kinnan and Townsend 2012) that indirect support links matter; e.g. h might support i so that i can support j, but we do not observe a link between h and j. Such link interdependencies cannot be incorporated well in dyadic regression analysis, yet might directly affect the results. As shown in Section 2.5.3.3, just

incorporating information on whether two households have a common friend already changes the estimation results considerably. This deficit of dyadic regression analysis might explain the unsatisfactory predictive power of the estimation models. Other models, such as exponential random graph models, are more able to capture observed network structures, but accommodate individual attributes only to a limited extend, which makes these models less suitable for testing specific predictions e.g. regarding household characteristics (Lusher et al. 2013).

In summary, this study highlights the potentials and limitations of analyzing the theoretical predictions of a reciprocal risk-sharing model with dyadic regression. I demonstrate the pivotal role of predicted neediness in a model of reciprocal risk-sharing and show that predicted neediness can explain part, yet not all, of the observed structure of mutual support arrangement. I emphasize the importance to account for the reports of both sides of a reported link and to consider the direction of the flow of support: I show that characteristics of reciprocated support links differ significantly from characteristics of unreciprocated support links; not taking these differences into account can lead to erroneous inference. Finally, throughout the chapter I discuss the methodological limitations of dyadic regression analysis to investigate link formation. This study can thus serve as a guidance for future research on support arrangements to apply adequate techniques of link elicitation and network analysis while being aware of the methodological constraints.

Chapter 3

Insurance and Solidarity

(with Susan Steiner)

3.1 Introduction

Given the dearth of insurance markets in developing countries, many governments, the private sector and the donor community have made considerable efforts over the last years to design appropriate insurance products and to expand people's coverage with formal insurance (Churchill and McCord 2012). A growing literature deals with the consequences for informal support – i.e., monetary transfers provided by relatives, neighbors and friends – when insurance is introduced (Attanasio and Rios-Rull 2000; Landmann et al. 2012; Boucher and Delpierre 2014; Lin et al. 2014; Klohn and Strupat 2015). This literature investigates the extent of crowding out: if insurance crowds out informal support and delivers only incomplete risk coverage (for example, because not everybody gets insured, insurance does not fully compensate losses, or not all risks are insurable), introducing insurance does not necessarily lead to welfare improvements. A clear understanding of the conditions under which crowding out occurs is thus necessary to help design insurance contracts that avoid such unintended consequences.

Attanasio and Rios-Rull (2000), Boucher and Delpierre (2014), and Lin et al. (2014) illustrate that insurance crowds out risk-sharing transfers, which aim at inter-temporal consumption smoothing. They identify the following mechanisms of crowding out. First, insurance increases the value of autarky and thus reduces people's commitment to risk-sharing (Attanasio and Rios-Rull 2000; Lin et al. 2014); second, insurance payouts substitute risk-sharing transfers (Lin et al. 2014); and third, insurance encourages people to take more risk, which is counteracted by a reduction in risk-sharing in order to curtail excessive risk-taking (Boucher and Delpierre 2014). The focus of these studies is the effect of insurance on risk-sharing transfers through changes in economic incentives. Social preferences are assumed to be unaffected. In this study, we consider the possibility that insurance affects social preferences. We hypothesize that social preferences are context dependent – i.e., that they determine different levels of informal support with and without insurance available. We focus on a different type of transfers: solidarity transfers, which

are informal transfers solely motivated by social preferences.

We report results from a lab-in-the-field experiment that investigates the effect of insurance on solidarity transfers. We designed a novel game, the transfer game, that borrows both from the dictator game and the solidarity game. Players are randomly assigned the role of provider or recipient. Each provider is anonymously matched with one recipient. Both receive the same endowment. The recipient can lose a large proportion of her endowment due to a random idiosyncratic shock. We vary whether the recipient has the option to purchase an insurance which avoids the loss from the shock. The provider is asked how much of her endowment she would transfer in case the recipient loses. Transfers are only enacted if the recipient experiences the shock. The transfer game is a one-shot game, in which transfers cannot be driven by economic incentives.

There is considerable evidence from behavioral economics suggesting that people condition their solidarity transfers to another person on this person's prior choices.[1] In particular, people are found to reduce their solidarity transfers when the other person could have avoided her neediness - by participating in the labour market (Fong 2007), by choosing a safe amount over a risky lottery (Trhal and Radermacher 2009; Cappelen et al. 2013a), by choosing a lottery that is less risky than another (Bolle and Costard 2015), or by purchasing insurance (Mollerstrom et al. 2015).[2] Given that all these studies were conducted with students in university labs of high-income countries, it is to be determined whether their insights on conditioning transfers to choice can be repeated among populations of developing countries. Decisions and actions taken by these populations may be inherently different from those of students in rich countries due to different norms, frames, past experiences and lessons learned from these experiences (Levitt and List 2007). We are aware of only one related study providing evidence from a developing country: Morsink (2016) shows that subjects in Ethiopia condition their solidarity transfers on the riskiness of recipients' choices.

We conduct our experiment with villagers in Cambodia, a country with an under-developed but growing insurance market (UNDP 2013; Microinsurance Network 2016). The Cambodian government, international donors, private insurance companies and non-governmental organizations are engaged in establishing different forms of social and private insurance, which makes the setting particularly relevant. The villagers who participated in our experiment are currently not very familiar with insurance but will likely face an increasing amount of insurance options in the near future. We play with high stakes to mimic substantial losses for which support from other people becomes necessary. The average payout in our experiment amounts to more than twice the daily average per capita income in rural Cambodia.

In addition to analyzing whether solidarity transfers are reduced when insurance becomes available, we also investigate if the reduction is affected by whether the individual in need was

[1] There is also substantial survey-based evidence along these lines. By linking beliefs about the sources of inequality to preferences for governmental redistribution, it was shown that individuals who perceive inequality to be the result of luck rather than of effort or of deliberate choice are most supportive of redistribution (Fong 2001; Corneo and Grüner 2002; Alesina and Angeletos 2005).

[2] The study by Mollerstrom et al. (2015) differs greatly from the present study. The authors implement games in which they ask third-party spectators to equalize incomes between two subjects. The income inequality is either the result of brute bad luck or of bad luck that could have been avoided by purchasing insurance.

aware of the possibility to receive transfers by someone else when making her insurance choice. Recent studies show that, under certain circumstances, individuals deliberately do not take up insurance and instead free-ride on the support of their peers (De Janvry et al. 2014; Janssens and Kramer 2016). We here analyze how the peers react to such free-riding. We argue that if individuals know that solidarity transfers may be forthcoming, the foregoing of insurance can be interpreted by peers as free-riding on their solidarity. If, in contrast, individuals do not know about potential solidarity transfers, such an interpretation is implausible. We thus expect that people respond to an informed decision against insurance with a stronger withdrawal of solidarity transfers. Previous studies on conditioning transfers to choice do not allow for disentangling such an information effect. In these studies, all subjects were either informed (Trhal and Radermacher 2009; Cappelen et al. 2013a; Bolle and Costard 2015) or uninformed (Fong 2007; Mollerstrom et al. 2015) about the later redistribution before they made their choices. In our experiment, we thus vary whether the recipient is informed about the potential transfer from the provider.

In a first step, we investigate the prevalence of *choice conditionality*; i.e., to what extent does the provider reduce her transfers when the recipient could have avoided her loss by purchasing insurance. In a second step, we analyze the *information effect*; i.e., does the provider react differently to the insurance option depending on whether the recipient is aware of the provider's potential transfer. The providers in our experiment transfer, on average, 13% of their endowment to recipients who lost most of their endowed money and had no option to insure. We find a significant reduction in transfers when the recipients can be held accountable for their neediness: providers reduce their transfers by 28%, on average, when recipients forewent the option to insure. This result confirms the conditionality found in previous studies; in fact, the proportion of individuals who condition their solidarity transfers on peers' choices is similar among our Cambodian villagers and the student populations of these studies. To our surprise, providers equally reduce their transfers towards recipients who were informed about the potential transfer from providers and towards recipients who were not informed. Hence, the reduction of solidarity transfers in response to insurance does not depend on the level of information available to the individuals in need. In other words, free-riding is not sanctioned; in fact, foregoing insurance may not be perceived as free-riding.

We provide evidence that solidarity transfers vary with the availability of insurance. We argue that this is due to the fact that the underlying social preferences are context dependent: people apply different norms of solidarity with and without insurance available. Different levels of solidarity transfers are thus provided to people in need depending on whether these people had the option to get insured. Our findings have important implications for the emerging literature on the interplay between informal support and formal insurance. First, we illustrate that insurance may not only crowd out risk-sharing transfers but also solidarity transfers. The mechanism of the crowding out is different from those suggested in the existing literature. Second, we point out that social preferences are context dependent. In future crowding out studies, we consider it necessary to determine whether the informal support of interest is motivated by social preferences, economic incentives, or both; and, if adequate, to allow for social preferences to determine different levels of transfers when insurance gets introduced. In the case of risk-sharing transfers, we expect that

insurance affects both social preferences and economic incentives and that these motives interact with each other in a complex way. How exactly they interact needs to be determined.

The remainder of this chapter is structured as follows. In Section 3.2, we introduce the transfer game and present the experimental design. We derive two hypotheses. First, people condition their solidarity on the choices of their peers (Hypothesis 1). And second, people condition their response to the choices of the peers on the level of information that the peers have about the support they may receive (Hypothesis 2). In Section 3.3, we describe the implementation of the experiment in the field. The results are presented in Section 3.4. We test Hypothesis 1 and Hypothesis 2, separately. We first present average treatment effects and then turn to the heterogeneity in the treatment effects. Lastly, we address the external validity and the plausibility of the assumed preferences. Section 3.5 concludes.

3.2 Conceptual Framework and Experimental Design

3.2.1 The Transfer Game

We designed a game, which we call the transfer game, to investigate the extent to which people condition their solidarity transfers on the choices of others. The transfer game is a one-shot game. There are two players, the provider and the recipient, with incomes x and y, respectively. The players have the same initial income – i.e., $x^e = y^e$. However, the recipient faces the possibility of an income shock that occurs with probability π; with $0 < \pi < 1$. If a shock occurs the recipient's income is reduced to y^s, with $y^s < y^e$. The provider does not face income shocks. In case the recipient experiences a shock, the provider can decide to transfer part of her income, T, to the recipient (with $0 \leqslant T \leqslant x^e$).

In the transfer game, treatments vary in two dimensions:

1. Information of the recipient

   The recipient is uninformed about the provider
   The recipient is informed only about her own role in the game. She is not informed about the existence of the provider who might transfer to her in case of an income shock.

   The recipient is informed about the provider
   At the beginning of the game, the recipient is informed about the existence of the provider. She is also informed that the provider has the possibility to transfer to her in case she experiences an income shock.

2. Option of insurance

   The recipient has no insurance option
   The recipient has no option to avoid the potential loss. Her income is y^s in case the shock occurs and y^e in case no shock occurs.

The recipient has an insurance option
Before the shock is determined, the recipient has the option to purchase insurance which covers the loss resulting from the income shock.

The price of insurance is p with $p \leq \pi \cdot (y^e - y^s)$; i.e., insurance is not more expensive than the actuarially fair price. If the recipient purchases insurance, her income is $y^e - p$ independent of whether a shock occurs or not. If the recipient does not purchase insurance, her income is y^e in case no shock occurs and y^s in case the shock occurs. Note that the provider always has full information and that a transfer from the provider to the recipient can only take place if the recipient's income is reduced to y^s. Combining the information and the insurance treatments, there are four different states in which the recipient's income can be reduced to y^s and a transfer can take place. These states and the corresponding transfers are depicted in Figure 3.1. Case 1 describes the two states in which the recipient has no information about the provider; Case 2 the two states in which the recipient does have information. We refer to the provider in Case 1 as $A1$ and to the provider in Case 2 as $A2$. The recipient is called $B1$ (without insurance option) or $B2$ (with insurance option) in Case 1 and $C1$ (without insurance option) or $C2$ (with insurance option) in Case 2.

In Case 1, the provider is asked to make two strategic transfer decisions:

1. The amount she would transfer to $B1$ in case this recipient experiences an income shock and is left with y^s [T^{00}].
2. The amount she would transfer to $B2$ in case this recipient experiences an income shock and is left with y^s [T^{01}].

The provider is then randomly matched with either $B1$ or $B2$. If the matched recipient indeed experiences a shock, the respective transfer decision is implemented.

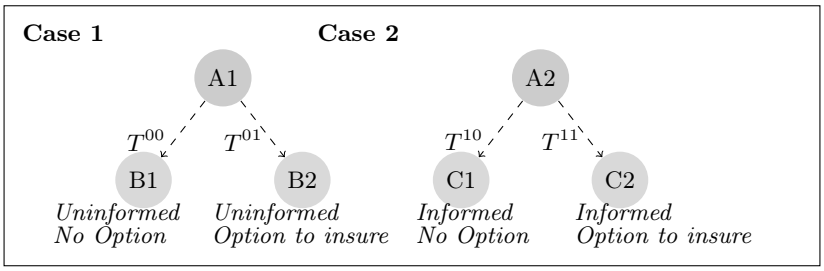


Figure 3.1: Transfer Decisions of Providers

In Case 2, the provider is asked to make the following strategic transfer decisions:

1. The amount she would transfer to $C1$ in case this recipient experiences an income shock and is left with y^s [T^{10}].
2. The amount she would transfer to $C2$ in case this recipient experiences an income shock and is left with y^s [T^{11}].

The provider is then randomly matched with either $C1$ or $C2$. If the matched recipient experiences a shock, the respective transfer decision is implemented.

We assume that when making her transfer decisions the provider is motivated by her own income as well as the desire to behave in line with her solidarity norm. We specify her utility as follows:[3]

$$U(\cdot) = v(x - T) - f(\phi - T) \tag{3.1}$$

$v(\cdot)$ is the provider's utility from her material payoff after the transfer, with $v'(\cdot) > 0$ and $v''(\cdot) < 0$. ϕ describes the provider's solidarity norm, which specifies the amount that the provider perceives to be the adequate transfer to the recipient. This solidarity norm can be affected by both personal and social norms. It depends on the income allocation, (x, y), and the context under which this income allocation was achieved – i.e., the information and insurance status of the recipient. $f(\cdot)$ describes the cost that the provider incurs when her transfer T deviates from the level of solidarity she perceives as adequate. Following the literature (Cappelen et al. 2007; Konow 2010), we assume $f'(\phi - T) \cdot (\phi - T) > 0$ for $\phi \neq T$, and $f''(\cdot) > 0$. The provider maximizes her utility with respect to T. With the assumed utility specification in Equation 3.1, the provider's solidarity norm has a direct impact on her optimal transfer decision, as $0 < \frac{dT^*}{d\phi}|_{x=cons.} < 1$ (see Konow 2010). This implies that the underlying solidarity norm determines the transfer provided.

We allow for the provider's solidarity norm to differ across the four states in the transfer game. We refer to the solidarity norms as ϕ^{00}, ϕ^{01}, ϕ^{10} and ϕ^{11} in line with the resulting transfers. If solidarity norms differ across states, this must necessarily result from the variation in the context, namely the four combinations of the information and insurance status, because the income allocation that initiates a transfer from the provider to the recipient is always (x^e, y^s). This implis that differences in solidarity norms describe the extent to which the provider conditions her solidarity on the information and the insurance option of the recipient.

1. Choice Conditionality
   Individuals may or may not differ in the extent to which they condition their solidarity on other individuals' choices (regardless of whether the choices were made informedly or not). We differentiate between the following solidarity types:

   1i) Unconditional solidarity. The provider's level of solidarity is unconditional on whether or not the recipient could have avoided her loss. Hence, the transfer that the provider perceives as adequate in case the recipient's income is reduced to y^s is not affected by the recipient's option to purchase insurance.

$$\phi^{00} = \phi^{01} \qquad \textit{if recipient not informed}$$
$$\phi^{10} = \phi^{11} \qquad \textit{if recipient informed}$$

[3] We assume a specification of social preferences that is common in the literature, modeled as a trade off between self-interest and fairness norms (e.g. see Cappelen et al. 2007; Konow 2010; Cappelen et al. 2013a).

1ii) Choice conditional solidarity. The provider's level of solidarity is conditioned on the recipient being able to avoid her loss. The transfer that the provider perceives as adequate in case the recipient's income is reduced to y^s depends on whether the recipient had the option to purchase insurance or not.

$$\phi^{00} \neq \phi^{01} \qquad \textit{if recipient not informed}$$
$$\phi^{10} \neq \phi^{11} \qquad \textit{if recipient informed}$$

2. Choice Conditionality and Information
Furthermore, we allow individuals' choice conditionality to depend on whether or not a choice was made informedly. We differentiate between the following types:

2i) The level of choice conditionality is independent of the recipient's information about the transfer possibility. The provider does not respond differently to the choice of the recipient when the recipient made her choice informedly compared with when the recipient made her choice uninformedly.

$$\phi^{01} - \phi^{00} = \phi^{11} - \phi^{10}$$

2ii) The level of choice conditionality depends on the recipient's information about the transfer possibility. The provider responds differently to the choice of the recipient when the recipient made her choice informedly compared with when the recipient made her choice uninformedly.

$$\phi^{01} - \phi^{00} \neq \phi^{11} - \phi^{10}$$

We do not observe the provider's type directly. However, because solidarity norms affect optimal transfers, the observed transfers from the provider to the recipient provide an indication of the provider's type. We can thus analyze the prevalence of the mentioned types by analyzing the differences in actual transfers T when the context of information and insurance option is varied.

The difference between T^{00} and T^{01} is the change in transfers when an uninformed recipient could have avoided the loss by purchasing insurance. The difference between T^{10} and T^{11} is the change in transfers when the recipient could have avoided the loss by purchasing insurance *and* made the choice to forego insurance informedly. Assuming the utility of the provider follows Equation 3.1, $T^{11} - T^{10}$ reflects the extent to which the providers' solidarity is conditioned on the *informed* choice of the recipient, and $T^{01} - T^{00}$ reflects the extent to which the providers' solidarity is conditioned on the *uninformed* choice of the recipient, or the choice *per se*. Based on previous findings on choice conditionality (e.g. Mollerstrom et al. 2015), we expect that the provider disapproves foregoing insurance. We should thus observe the provider to transfer less to $B2$ than to $B1$ in Case 1 and less to $C2$ than to $C1$ in Case 2 (see Table 3.1).

Table 3.1: Transfer Decisions and Hypotheses

		Recipient informed of provider	
		no	***yes***
Recipient had option to insure	***no***	T^{00}	T^{10}
	yes	T^{01}	T^{11}
Hypothesis 1		$T^{01} - T^{00} < 0 \quad T^{11} - T^{10} < 0$	
Hypothesis 2		$T^{01} - T^{00} > T^{11} - T^{10}$	

Hypothesis 1 - Choice Conditionality
Providers condition their solidarity on choice, in particular

$$\phi^{01} < \phi^{00} \qquad \textit{if recipient not informed}$$
$$\phi^{11} < \phi^{10} \qquad \textit{if recipient informed.}$$

The average provider sends a lower transfer to a recipient who forewent the option to insure than to a recipient who had no option of insurance (regardless of the level of information of the recipient); i.e.,

$$T^{01} - T^{00} < 0\,, \qquad \textit{if recipient not informed}$$
$$T^{11} - T^{10} < 0\,, \qquad \textit{if recipient informed.}$$

The difference in the transfer differences, $(T^{01} - T^{00}) - (T^{11} - T^{10})$, then reflects to which extent the provider's choice conditionality depends on the level of information available to the recipient. Whereas an uninformed recipient is not aware that she might receive a transfer from the provider when she foregoes insurance, an informed recipient has full information. The provider may thus perceive the informed foregoing of insurance of the recipient as deliberate reliance on her support, or as free-riding on her solidarity, and may not approve of this intention. In line with Falk and Fischbacher (2006)'s theory of intention-based reciprocity, we hypothesize that the provider's solidarity norms are influenced by distributional outcomes as well as by intentions; hence, the provider does not necessarily withdraw her support to an informed recipient who foregoes insurance completely. We merely expect that the provider in Case 1 reduces her transfer by more in response to foregoing insurance than the provider in Case 2.

Hypothesis 2 - Information Effect
Providers condition their response to choice on the recipients' level of information; in particular

$$\phi^{01} - \phi^{00} > \phi^{11} - \phi^{10}.$$

The average provider reduces her transfer to a recipient who forewent the option to insure informedly by more than to a recipient who forewent the option to insure uninformedly; i.e.,

$$T^{01} - T^{00} > T^{11} - T^{10}.$$

3.2.2 Experimental Procedure

We conducted the experiment in 21 villages (one session per village) in Cambodia. In each village, the experiment was run with 32 subjects: 16 providers and 16 recipients. There were two groups of providers (with 8 subjects per group) and four groups of recipients (with 4 subjects per group). All subjects played two rounds of the transfer game. Before the game was played, subjects were randomly allocated to one of the six groups; the group determined the role each subject would play in Round 1 and Round 2 (see Table 3.2). In Round 1, one of the provider groups ($A1$) played the game of Case 1 – i.e., with uninformed recipients – and the other provider group ($A2$) played the game of Case 2 – with informed recipients. In Round 2, providers switched the roles. This means that all providers made all four transfer decisions depicted in Figure 3.1 over the course of the two rounds. The recipient groups played the role of player $B1$, $B2$, $C1$ and $C2$ in Round 1 and the role of player $B2$, $B1$, $C2$ and $C1$ in Round 2, respectively.

Table 3.2: Overview of Player Roles

	Groups of Providers		**Groups of Recipients**			
	1	2	3	4	5	6
Round 1						
Role	**A1**	**A2**	**B1**	**B2**	**C1**	**C2**
Transfer decisions	T^{00} and T^{01}	T^{10} and T^{11}	-	-	-	-
Information	-	-	no	no	yes	yes
Insurance option	-	-	no	yes	no	yes
Round 2						
Role	**A2**	**A1**	**B2**	**B1**	**C2**	**C1**
Transfer Decisions	T^{10} and T^{11}	T^{00} and T^{01}	-	-	-	-
Information	-	-	no	no	yes	yes
Insurance Option	-	-	yes	no	yes	no
No. of subjects per session	8	8	4	4	4	4
Total no. of subjects (21 sessions)	168	168	84	84	84	84

Each group played in a separate room and subjects only observed the treatment of the group they belonged to. Neither communication nor interaction between the subjects within a room and between the rooms was allowed. Subjects were at no time told the purpose of the experiment and no feedback was provided to the subjects between the rounds. The experiment was implemented in an anonymous setting. Subjects did not know the identity of the subjects they were matched with;[4] and the research assistants supervising the games did not observe the subjects' decisions.

[4]Subjects saw each other during introduction before the game, but they did not know who played which role except for those who were in the same room. Thus, if providers wished to form expectations about the identity of the recipient, they had to take into account the pool of all subjects who were not in their group.

Those research assistants that could link the subjects' decisions in the game to their identity did not interact with the subjects until the final payout. This setting was explained during the introduction.
The parameters of the game were specified as follows:

- Initial Income $x^e = y^e = 16,000$
- Probability of shock $\pi = 0.5$
- Income after shock $y^s = 2,000$
- Price of insurance[5] $p = 6,000$

Figure B.1 in Appendix B.1 illustrates the resulting outcome tree for the transfer game.

The detailed procedure for recipients **B1**, **B2**, **C1** and **C2** was as follows:

1. Each recipient received an initial income of 16,000 Riel in sixteen 1,000 Riel bills in play money.[6]
2. Recipients were explained that each of them would roll a dice. The outcome would determine how much they could keep of the initial income. If the dice showed 1, 2 or 3, they would lose 14,000 Riel; if the dice showed 4, 5 or 6, they would keep the 16,000 Riel.
3. Recipients of type $B2$ and recipients of type $C2$ were explained the insurance option: they had the option to purchase a private insurance for the price of 6,000 Riel.[7] If a recipient decided to purchase the insurance, she would keep 10,000 Riel independent of the outcome of the dice.
4. Recipients of type $C1$ and recipients of type $C2$ were informed that each of them was matched with a player in a different room, that these players had a safe endowment of 16,000 Riel, but could decide to transfer part of it to their partner (i.e., to the $C1$ and $C2$ recipients) in case this person lost.
5. Each recipient was asked questions to test her understanding of the game.
6. Each recipient of type $C1$ and type $C2$ was asked to note down how much transfer she expected from her partner player in case of loss. The beliefs were noted down in private behind a cardboard and then collected. Recipients were told that their partner would never see these beliefs and that the beliefs had thus no impact on the actual transfer decisions.

[5]Note that this price is below the actuarially fair insurance price which would be 7,000 Riel. We intended to put a value on the insurance such that in expectation 50% of the recipients would purchase insurance. Pilot tests conducted with villagers as well as students in Cambodia suggested a price of 6,000 Riel for 50% uptake. However, in the actual experiment insurance uptake was higher.

[6]4,000 Riel are worth approximately 1 USD. As a benchmark: The average per capita income in rural Cambodia was about 2 USD in 2014, according to the Cambodia Socio-Economic Survey (National Institute of Statistics 2015). For the participants in our experiment, the average daily income per household was slightly more than 5 USD (including the income of all household members, remittances, state assistance etc.), the median daily household income was below 2 USD.

[7]For the insurance option, we intentionally did not use the Khmer word for 'insurance' but the more general word *'bankapie'* ('guarantee') in order to not evoke any associations with existing insurance schemes.

7. Recipients of type $B2$ and recipients of type $C2$ were asked to go outside the room one by one to make their insurance purchase decision with a research assistant sitting outside.[8] Recipients were not allowed to reveal their decision to the others when they came back into the room.
8. Each recipient rolled the dice. The outcome was noted down. In case a recipient lost, she handed 14,000 Riel of her play money to a research assistant. The remaining money was inserted in an envelope and collected; recipients were told that this money would be transferred to their personal 'game account.' This money together with any potential transfer of the provider determined the payout of the recipients for this round.

Then groups switched rooms and roles for the second round.

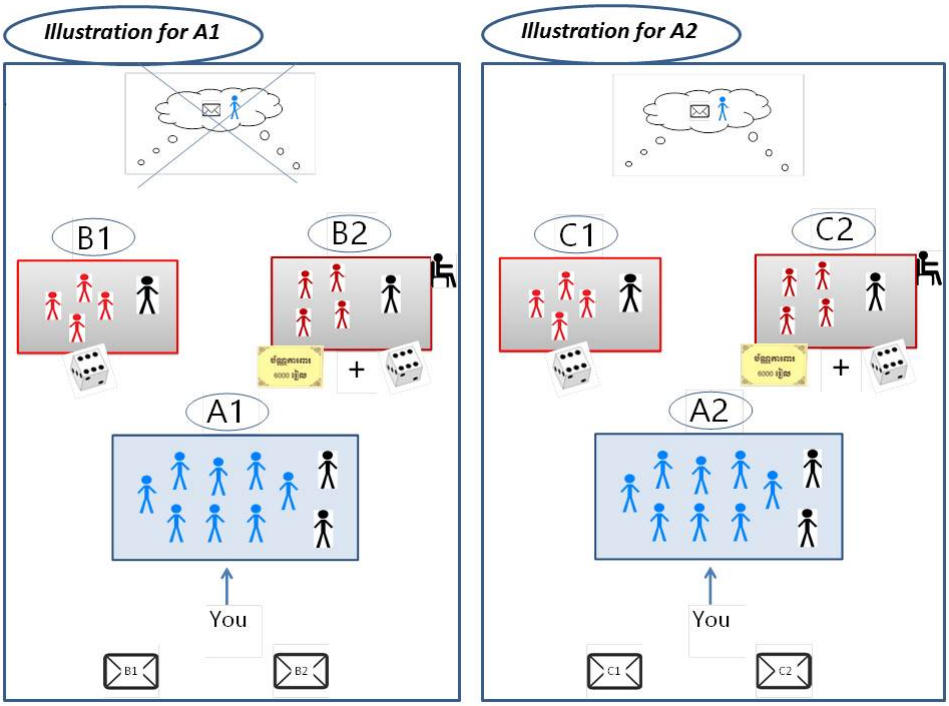


Figure 3.2: Illustrations for A1 and A2 Providers

[8]Note that this was the only decision that was not made in private. However, the research assistants responsible for the insurance sale were not part of the team of research assistants who supervised and explained the game, and they had not interacted with the subjects before.

The procedure for providers **A1** and **A2** was as follows:[9]

1. Each provider received an initial income of 16,000 Riel in sixteen 1,000 Riel bills in play money.
2. Providers were explained the situation of the recipients. Specifically, providers of type $A1$ were explained the situation of $B1$ and $B2$ recipients, and providers of type $A2$ were explained the situation of $C1$ and $C2$ recipients. Providers were shown one of the overview illustrations depicted in Figure 3.2 as well as a detailed illustration for each player type (see Figures B.2-B.5 in Appendix B.2.4).
3. Providers simulated the situation of the recipients, first of type $B1$ [$C1$], then of type $B2$ [$C2$]. During this simulation, each provider was asked questions to test her understanding of the game.
4. Providers were explained the random partner matching and the following transfer procedure. It was emphasized that transfers would only take place in case the partner lost money after rolling the dice and, for a recipient of type $B2$ and $C2$, had not bought insurance. Again, each provider was asked questions to test her level of understanding of the transfer procedure.
5. Each provider was asked to write down in private (behind cardboards) on two separate sheets the following transfer decisions (see decision sheets in Figures B.6-B.9 in Appendix B.2.5):

   - In case your partner was of type $B1$ [$C1$] - how much of your 16,000 Riel would you transfer if your partner loses?
   - In case your partner was of type $B2$ [$C2$] - how much of your 16,000 Riel would you transfer if your partner loses?

6. After decisions were noted down, providers had time to check both decisions and to make final changes; then, pencils were collected.
7. Each provider was asked to draw an envelope from a box. On the envelope was a sign indicating the player type of the partner and a unique ID for the partner (unidentifiable to the providers). Each provider was asked to insert into the envelope the relevant decision sheet and the amount of bills she had noted on the sheet.[10]
8. Providers were given a second envelope in which they placed the remaining amount of bills. They were told that this money would be transferred to their personal 'game account' and that in case their partner had not lost they would also receive back the amount they had transferred. This money would determine their payout for this round.
9. All decision sheets and envelopes were collected by the research assistants.

[9] For the script of the instructions for providers $A1$ and $A2$, see Appendix B.2.2 and B.2.3, respectively.

[10] Providers were told that the amount they inserted would be double-checked with the amount indicated on the decision sheet and that, in case there was a difference, the amount indicated on the decision sheet would determine the transfer.

Then the two provider groups switched rooms and roles for the second round. They did not receive any feedback about the actual outcome of their partner. The procedure of Round 2 was the same as in Round 1. Only the simulation of the recipients' situation and the related test questions for the providers were skipped.

3.3 Implementation of the Experiment in the Field

We ran the experiment between August and October 2015. The 21 experimental villages are located in Banteay Meanchey and Siem Reap provinces in Northwestern Cambodia (see Figure 3.3).[11] Two weeks before the experiment took place in a village, a detailed household survey was conducted with approximately 60 randomly selected households of the village as well as a community survey with the village head. In total, 1,272 households were interviewed. The survey focused on basic socio-economic information, employment, support networks within and outside the village, labor migration, access to formal risk management tools, such as insurance, savings and credit, as well as perceptions of solidarity and accountability.

At the end of each interview, the respondent was asked whether he or she was able and willing to participate in an upcoming experiment. If the respondent answered affirmatively, he or she was included in the pool of potential experimental participants for this particular village. Our original target participant was literate and between 18 and 65 years old. However, as the literacy rate in this region is very low and labor migration of the young in some villages particularly high, illiterate and older respondents had to be included. We sorted the list of potential participants according to their age and literacy level (youngest and most educated first) and sampled from this list in the resulting order. Thus, our group of experimental participants is not representative of the village population, the participants are younger and more educated than the average villager. For detailed characteristics of the experimental participants, see Appendix B.3.1.

The experiment took place in a school building either in the village itself or in a neighboring village. It was conducted with the assistance of 10 Khmer research assistants, who were different from those who had conducted the household survey. The experiment had five parts: registration and introduction; the first game (with two rounds) which is the focus of this study; a network questionnaire and a short break; a second game (with four rounds); closing remarks and payout.

At the registration, each participant drew blindly a participant badge from a bag: a colored card with a number from 1 to 32, the participant ID. The color determined the group the participant was allocated to.

After the registration, all participants were gathered in one room for a brief introduction where the general rules of the games and the payout modalities were laid out and the research team was introduced (for instructions for the introduction, see Appendix B.2.1). In particular, participants were explained that each of them would receive a show-up fee of 4,000 Riel; and that they could earn additional money over the course of the experiment which consisted of

[11] The experiment was conducted in eleven villages in Siem Reap province and in ten villages in Banteay Meanchey province. Villages were selected to be comparable within and across the two provinces. Selection criteria included the size of the village, the level of migration, and remoteness.

Figure 3.3: Map of Cambodia (Source: United Nations 2004)

several rounds. How much they would keep at the end of each round would be dependent on their luck, their choices and the choices of others. Participants were told that they would not receive any feedback between the rounds. At the end, only one round would be selected for payout by the draw of a ball; hence, their decisions in one round should not be affected by their decisions or their outcomes in other rounds. Participants were ensured that their decisions would be kept anonymously and would not be observed by any of the other participants or the research assistants they interacted with. Participants were told that they were not allowed to communicate with each other during the course of the experiment, and that if they disobeyed the rules they would need to leave. After making sure that the rules were understood, the participants split into their groups according to the colors of their participant badges and were accompanied by the research assistants to their rooms.

The first game was conducted as described in Section 3.2.2. The four rooms with the recipients were each supervised by one research assistant (with two additional assistants sitting outside the room for insurance sale), the two rooms with the providers by two research assistants, respectively. The explanation of the game was done in front of all participants of each group. However, participants wrote their decisions in private and unobserved by the research assistants behind cardboard boxes. Although the literacy rate was low, most of the participants could read and write numbers. 13% of the providers needed help from the research assistants in writing their transfer decisions. We take this into account in the analysis. Low literacy constituted a challenge for us to explain the game in such a way that it could be understood by the participants. We employed several measures, such as using graphical illustrations and simulating the role of the other players, to increase the level of understanding. We also asked different sets of test questions during the instructions, the results of which can be regarded as an indicator for the level of understanding. Given that we are interested in the providers' behavior, it is important to us that they clearly understood the game and the implications of their transfer decisions. 44% of

the providers gave correct answers to all test questions, and another 22% made only one mistake. Part of the analysis below will be restricted to these providers.

At the end of the experiment, all participants were gathered in one room for the closing remarks. One participant was asked to draw blindly one ball from a bag that contained six different balls (one for each round). The drawn ball determined which round would be paid out. The participants were then asked one by one to a separate room where they received their payout which consisted of the outcome of the round that was drawn and their show-up fee. The average payout amounted to 17,000 Riel, equivalent to 4.25 USD or slightly more than twice the average per capita income in rural areas (National Institute of Statistics 2015). The full experiment lasted, on average, 4.5 hours.

3.4 Results

3.4.1 Treatment Effect Analysis

Over Round 1 and Round 2, each of the 336 subjects who played the role of the provider made four transfer decisions, two as an $A1$ provider (to an *uninformed* recipient with and without insurance option) and two as an $A2$ provider (to an *informed* recipient with and without insurance option). Hence, there are 1,344 observations in total (4×336). In the empirical analysis below, we pool the transfer decisions of the $A1$ and $A2$ providers.[12]

Figure 3.4 and Figure 3.5 depict the frequency distribution of the transfer decisions. We separately show the transfer decisions to recipients who were uninformed about the transfer possibility (Figure 3.4) and to those who were informed (Figure 3.5). For simplicity, the transfers are divided by 1,000 in these figures and in all following tables. The amount of transfer varies considerably; with the majority of providers transferring 1,000 or 2,000 Riel. Only a very small number of providers are willing to transfer 7,000 Riel, which would result in an equal split of the endowment such that both provider and recipient ended up with 9,000 Riel.[13] There is a considerable shift to zero transfers when the recipients had the option to insure: the number of $A1$ providers who transfer zero when insurance is available increases more than threefold, the number of $A2$ providers more than fivefold. The distribution of transfers to uninformed recipients is not much different from the distribution of transfers to informed recipients, with a slightly higher proportion of $A1$ providers transferring zero.

In order to test for the prevalence of choice conditional solidarity and the information effect as outlined in Hypothesis 1 and Hypothesis 2 we exploit the within-subject and orthogonal

[12] We compare the socioeconomic characteristics of the $A1$ and $A2$ provider groups in Table B.2 in the Appendix B.3.1. We find no overall significant differences, with the only exception of *bank account*. This is an indication that randomization was successful and that we can pool the transfer decisions for the analysis.

[13] This fact makes our results different from Cappelen et al. (2013a) and Mollerstrom et al. (2015) who observe a tendency of the providers to equalize income. It is important to note that our experimental design is quite different from that of Cappelen et al. (2013a) and Mollerstrom et al. (2015). These studies rely on dictator games, in which dictators or third-party spectators have to decide how to distribute the *total* amount of two incomes (i.e., of the dictator and of the recipient). It is likely that this procedure makes the norm of an equal split more salient. In contrast, studies that use solidarity games, rather than dictator games, to analyze choice conditionality, also do not observe an equalization of incomes (Bolle and Costard 2015; Trhal and Radermacher 2009).

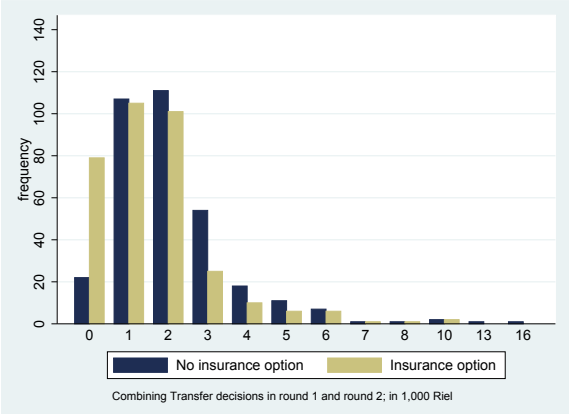


Figure 3.4: Transfer Distribution for Providers A1

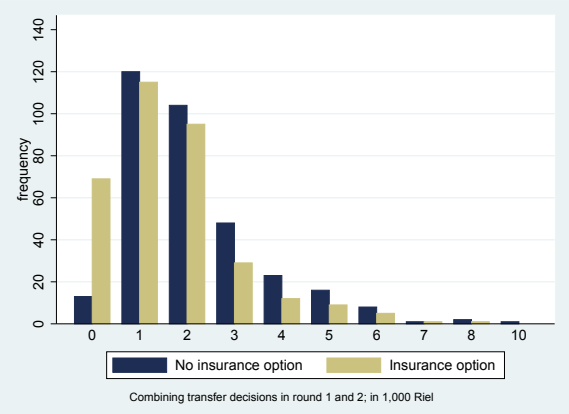


Figure 3.5: Transfer Distribution for Providers A2

treatment design and specify the transfer decision of provider i in treatment t as follows:

$$transfer_{i,t} = \theta + \beta Inf_t + \gamma Opt_t + \eta InfOpt_t + \epsilon_{i,t} \tag{3.2}$$

Opt_t is equal to one if recipients had the option to insure and zero otherwise. Inf_t is equal to one if recipients were informed about the transfer possibility and zero otherwise. $InfOpt_t$ is an interaction term, equal to one if recipients were informed about the transfer possibility *and* had the option to take up insurance and zero otherwise. We include a dummy to control for round effects – i.e., level changes in transfer decisions when providers played the transfer game a second time.

We can break down the transfer decisions of providers as depicted in Table 3.3 (mirroring Table 3.1). In line with Hypothesis 1, we expect $\gamma < 0$ and $\gamma + \eta < 0$; and in line with Hypothesis 2, we expect $\eta < 0$.

Table 3.3: Transfer Decisions and Regression Coefficients

		Recipient informed of provider	
		no	***yes***
Recipient had option to insure	***no***	θ	$\theta + \beta$
	yes	$\theta + \gamma$	$\theta + \beta + \gamma + \eta$
Hypothesis 1		$\gamma < 0$	$\gamma + \eta < 0$
Hypothesis 2		$\eta < 0$	

We estimate Specification 3.2 using OLS with standard errors clustered at the village level

(using the wild cluster bootstrap).[14] To account for corner solution response and for unobserved heterogeneity at the individual level, we also conduct a Tobit random effects estimation with the outcome variable censored at zero (see Table B.3 in Appendix B.4.1). We here report the results of the OLS estimation for the ease of interpretation.

The estimation results are reported in Table 3.4. In Column 1, we report the basic results without controlling for round effects. In Columns 2 – 6, we control for round effects. In Column 3, the sample is restricted to those providers who did not need support in writing their transfer decisions (292 out of 336 providers). In Column 4, the sample is restricted to providers who made none or only one mistake in the test questions which were asked before transfer decisions (227 providers). In Column 5, we further restrict to providers who answered all test questions correctly (149 providers). In Column 6, we exclude the 24 providers who made extreme transfer decisions; these are providers that indicated to transfer more than 7,000 Riel in at least one treatment.

Table 3.4: Treatment Effect Analysis - Pooled OLS

	(1)	(2)	(3)	(4)	(5)	(6)
	Transfer	Transfer	Transfer	Transfer	Transfer	Transfer
Inf (β)	0.003	0.003	0.008	0.014	0.092*	0.097
	(0.121)	(0.121)	(0.140)	(0.122)	(0.050)	(0.059)
Opt (γ)	-0.598***	-0.598***	-0.596***	-0.670***	-0.738***	-0.533***
	(0.102)	(0.102)	(0.107)	(0.102)	(0.115)	(0.081)
InfOpt (η)	0.018	0.018	-0.048	-0.013	-0.007	-0.024
	(0.078)	(0.078)	(0.097)	(0.078)	(0.058)	(0.053)
Constant (θ)	2.155***	2.275***	2.318***	2.360***	2.272***	2.117***
	(0.121)	(0.132)	(0.146)	(0.177)	(0.135)	(0.089)
Round effects	No	Yes	Yes	Yes	Yes	Yes
Observations	1344	1344	1168	908	596	1320
r2_a	0.034	0.039	0.039	0.052	0.074	0.046
F	15.146	20.016	19.414	20.438	26.259	19.701

Pooled OLS estimator; standard errors (wild cluster bootstrap) in parentheses.
(1)-(2) for all subjects; (3) excluding subjects who needed support in writing.
(4) excluding subjects who made at least two mistake at test questions.
(5) excluding subjects who made at least one mistake at test questions.
(6) excluding subjects who made at least one transfer above 7,000 Riel.
Transfers in terms of 1,000 Riel

The average baseline transfer (θ) – i.e. how much is sent to recipients who experienced a shock but had no insurance option and were not informed – is 2,155 Riel in Column 1. Hence, the providers transfer 13.5% of their endowment. Introducing the insurance option has a significantly negative effect on the transfers. Providers reduce their transfers to recipients who forewent the insurance option by 28% in case the recipient is uninformed (γ as a proportion of θ) and by 27% in case the recipient is informed ($\gamma + \eta$ as a proportion of $\theta + \beta$). Both γ as well as the joint

[14] Due to the orthogonal treatment design, fixed effects and random effects models come to the same results as standard OLS (Oaxaca and Dickinson 2005).

effect of $\gamma + \eta$ are significantly negative; a support of our first hypothesis. We find no evidence that the extent to which solidarity is conditioned on the insurance choice depends on the level of information available to the recipients. η is close to zero and statistically insignificant. On average, information does not seem to matter. There is neither a significant change in transfers in response to information per se (β) nor a change in the response to foregoing insurance (η). Introducing round effects in Column 2 and restricting the sample in Columns 3 – 6 does not change the results by much.[15] The baseline transfer varies between 2,100 Riel and 2,400 Riel (13%-15% of the endowment) across these columns. γ as well as $\gamma + \eta$ are always significantly negative; but η alone is statistically insignificant. The magnitude of γ and $\gamma + \eta$ amounts to between 25% and 32% and is thus comparable to that in Column 1.

A potential concern with these results arises from the fact that providers make their transfer decisions strategically: They know the transfer will only be enacted if recipients actually lose. In the transfer game, the probability of a transfer being implemented varies across the treatments with and without the insurance option. When recipients have no insurance option, there is a 50% probability of losing and thus of transfers. When recipients have an insurance option, the probability decreases as soon as some recipients take up insurance and thus have a zero probability of a loss, making transfers impossible. In our experiment, insurance uptake was relatively high, namely 75%. Hence, the probability of a transfer was 0.75*0+0.25*0.5=12.5% in the treatments with the insurance option. If providers expected high insurance uptake, they may have indicated a relatively high transfer amount in the treatments with the insurance option because they did not expect a transfer would take place. Such behavior would be particularly likely if providers were not only motivated by solidarity norms but also by a desire to 'look good' in the eyes of the research assistants, the other experimental participants or themselves; i.e., by their social or self image. As described in Section 3.2.2, we tried to limit concerns for social image by using an anonymous setting in which neither the research assistants nor the other participants could observe the transfer decisions. Nevertheless, providers might still be motivated by their self image (Dana et al. 2007). If this was the case, then differences in transfers would be larger in a non-strategic setting. The transfer differences that we observe should thus be interpreted as a lower bound for the true extent of choice conditionality.

In sum, we find support for Hypothesis 1, but no support for Hypothesis 2. On average, providers reduce their transfers significantly when recipients had the choice of insurance and hence the option to avoid the loss. The reduction in transfers to recipients who forewent the option to insure informedly is *not* larger than that to recipients who forewent the option to insure uninformedly. These results are stable across all columns in Table 3.4 (OLS results) and Table B.3 (Tobit random effects results). This finding is an indication that people tend to condition their solidarity on the choices of their peers and hold others accountable for prior decisions; yet it seems irrelevant whether or not the peers knew about the potential support before making their choice.

[15] There are round effects. Subjects reduce their baseline transfers from Round 1 to Round 2; yet, the treatment effect of the insurance option as well as the information remain unchanged. We analyze the rounds separately in Table B.4 in Appendix B.4.2.

3.4.2 Heterogeneous Treatment Effects

Are the observed average transfer changes driven by just a few providers in our sample or do they represent a pattern common to the whole provider sample? To answer this question, we investigate the distribution of treatment effects across providers. We first take a look at choice conditionality (γ and $\gamma + \eta$) and then look at the information effect (η). Five providers (i.e., 1.5%) transferred zero in each treatment. We exclude them from the following analysis because they behave purely payoff maximizing and do not show any sign of solidarity.

Table 3.5 depicts the distribution of the change in transfers to an uninformed recipient in response to foregoing insurance (γ). Table 3.6 shows the respective distribution for an informed recipient ($\gamma + \eta$). Column 1 reports the distribution for all providers; Column 2 restricts the sample to providers that made at most one mistake at the test questions; and Column 3 is for providers that responded correctly to all test questions.

Table 3.5: Heterogeneity in γ

	(1) Proportion (*all subjects*)	(2) Proportion (*Most test questions correct*)	(3) Proportion (*All test questions correct*)
$\gamma = 0$	42.30	43.05	45.21
$\gamma < 0$	44.71	47.53	47.95
$\gamma > 0$	12.99	9.41	6.85
Observations	331	223	146

Excluding subjects who always transferred zero

(2) excluding subjects who made at least two mistakes at test questions

(3) excluding subjects who made at least one mistake at test questions

Table 3.6: Heterogeneity in $\gamma + \eta$

	(1) Proportion (*all subjects*)	(2) Proportion (*Most test questions correct*)	(3) Proportion (*All test questions correct*)
$\gamma + \eta = 0$	42.90	43.50	41.31
$\gamma + \eta < 0$	44.71	47.09	51.37
$\gamma + \eta > 0$	12.39	9.42	7.53
Observations	331	223	146

Excluding subjects who always transferred zero

(2) excluding subjects who made at least two mistakes at test questions

(3) excluding subjects who made at least one mistake at test questions

Indeed, not all providers exhibit choice conditional solidarity. 45% of providers reduce their transfers when recipients had the option to insure ($\gamma < 0$ or $\gamma + \eta < 0$); in fact, these providers reduce their transfers by 65%. In contrast, 42% (Table 3.5) or 43% (Table 3.6) of providers do

not condition their transfers on the choice of recipients ($\gamma = 0$ or $\gamma + \eta = 0$). This means these subjects transfer the same amount independent of whether recipients had the option to insure. Interestingly, 13% (Table 3.5) or 12% (Table 3.6) of the providers transfer *more* when recipients forewent the insurance ($\gamma > 0$ or $\gamma + \eta > 0$). This proportion reduces to 9% when we restrict the sample to subjects who responded mostly correctly to the test questions and further to 7% when we restrict to only correct answers. This reduction suggests that some of the observations are caused by erratic decisions by providers that did not clearly understand the experiment.[16]

The proportion of providers who condition their transfers on the choices of others in our experiment is comparable to the proportions in Trhal and Radermacher (2009), Cappelen et al. (2013a), Mollerstrom et al. (2015) and Bolle and Costard (2015). In these studies, between one third and two thirds of experimental participants conditioned their transfers on the past decisions of the co-players. Hence, choice conditionality does not seem to be a phenomenon specific to high-income countries where the other studies were implemented but appears to be prevalent in developing countries to a similar extent.

The distribution of responses to the information of recipients (η) is illustrated in Table 3.7. Again, there is considerable heterogeneity. The majority of providers (61%) do not condition their response to foregoing insurance on the information available to recipients ($\eta = 0$); i.e., providers adjust their transfers by the same amount for informed and uninformed recipients. It seems that either they do not regard the informed recipients' behavior as free-riding on their solidarity or they do not consider that free-riding on solidarity should be punished.[17]

Table 3.7: Heterogeneity in η

	(1) Proportion (*all subjects*)	(2) Proportion (*Most test questions correct*)	(3) Proportion (*All test questions correct*)
$\eta = 0$	60.73	64.12	67.12
$\eta < 0$	20.24	17.45	17.12
$\eta > 0$	19.03	18.39	15.75
Observations	331	223	146

Excluding subjects who always transferred zero

(2) excluding subjects who made at least two mistakes at test questions

(3) excluding subjects who made at least one mistake at test questions

[16] In order to better understand the reasoning behind the transfer behavior, we conducted qualitative interviews with participants after the experiment in one third of the villages. Of the interviewed providers, 19 increased their transfers when the recipients had had the option to take up insurance. The majority of these providers seemed to not have understood the situation of the recipients or confused the order of the two decisions. Four providers stated they felt more pity with recipients who decided against the insurance and lost, than with recipients who just lost due to pure misfortune. One provider responded she expected the recipient would take up the insurance anyway and thus did not care about the transfer. And one provider stated he 'just did not care about the money.'

[17] In fact, the insurance uptake of the uninformed recipients is only slightly higher than that of the informed recipients: 79% vs. 71%. This indicates that there is some free-riding among recipients but not very much. If providers expect little free-riding, it seems reasonable that they do not make a difference between foregoing insurance informedly and foregoing insurance uninformedly.

20% of the providers behave in line with our Hypothesis 2 ($\eta < 0$). They reduce their transfers by *more* to recipients who forewent insurance informedly. Surprisingly, the proportion of providers that respond positively ($\eta > 0$), is approximately as high as the proportion of providers that respond negatively. 19% reduce their transfers by *less* when the recipients made their choice informedly. This share reduces only slightly to 18% (16%) when we restrict the sample to those providers that responded mostly correctly (only correctly) to all test questions. Thus, problems with understanding the experiment do not seem to be the root cause of this behavior. These providers may instead be driven by different motives than the others. Possibly, they wish to reward the foregoing of insurance of the informed recipients as they advocate the institution of informal support.[18] Such behavior is still in line with intention-based reciprocity but providers apply a positive, rather than a negative, reciprocal response. An indication for this interpretation may be that providers with $\eta > 0$ have considerably higher baseline transfers (θ) of 2,651 Riel compared with 2,150 Riel among providers with $\eta < 0$ and 1,701 Riel among providers with $\eta = 0$. Given that they are willing to make larger transfers than others, they seem to value informal support highly. An alternative motive is guilt aversion (Charness and Dufwenberg 2006; Battigalli and Dufwenberg 2007): providers may wish to comply with the expectations of recipients, who made the choice against the insurance in reliance on their support. With the data currently at hand, we cannot say anything about the plausibility of these interpretations. We did not elicit the perception of intentions or the second-order beliefs of the providers in our experiment. Hence, we do not know whether the providers regarded the recipients' insurance decision as intentionally good or bad and whether they took the recipients' expectations into account when making transfer decisions.

3.4.3 Supporting Evidence

Our experiment shows that, on average, providers condition their solidarity transfers on the insurance choice of their peers and do not vary their behavior according to the peers' awareness of potential monetary support. A number of features of our experimental design may raise concerns over the generalizability and interpretation of our findings. In the following, we address such concerns, in particular the external validity of the experiment and the validity of the presumed underlying preferences.

3.4.3.1 External Validity

In order to investigate our research question, we chose to conduct a lab-in-the-field experiment because it would have been difficult, if not impossible, to cleanly analyze choice conditionality in combination with the information effect through survey questions. A major limitation of lab experiments is, however, that they are artificial. As laid out by Levitt and List (2007), several characteristics of lab experiments make extrapolation of behavior in the lab to behavior outside the lab questionable. We designed our experiment in such a way that it eases extrapolation to the extent possible. For example, the decisions of the providers and recipients were not observed

[18] We thank Jean-Philippe Platteau for making us aware of this potential explanation.

by the research assistants who interacted with them. The level of scrutiny was thus minimized, which should have reduced the pressure to act pro-socially. Moreover, we played with high stakes, which may have further reduced the extent of unnatural pro-social behavior. Still, our subjects played anonymously and were not allowed to communicate with each other, which is far from real-life interactions.

We address the external validity of our experiment in two ways. First, we contrast the providers' transfer decisions in the experiment with their survey response to a vignette situation on accountability.[19] Second, we study the beliefs of the recipients in the experiment. In a first step, we analyze whether the recipients' beliefs about providers' transfers match the actual transfer decisions of the providers. In a second step, we correlate average recipients' beliefs and providers' transfer decisions per village.

Providers' behavior outside the lab

In the survey, we confronted the respondents with a vignette situation in order to analyze their perceptions of accountability in a situation which was familiar to them. Respondents were asked to consider two different situations:

- Situation 1 (S1): *"The household head of a poor household in this village dies of sudden illness. All other households are asked once to support the household. How much money would you contribute?"*
- Situation 2 (S2): *"The household head of a poor household in this village dies after a motorbike accident. He had behaved very risky on the road. All other households are asked once to support the household. How much money would you contribute?"*

80% of the providers report in the survey that they would contribute the same amount in both situations ($S1 = S2$). 19% report to provide less in Situation 2, where the outcome is the result of choice – i.e., careless driving ($S1 > S2$). Only 1% (4 providers) indicate to provide more in Situation 2.

Of course, the situation that individuals face in the experiment is very different from the situation they face in the survey. Death is a much more severe circumstance than the loss of money due to the roll of a dice. Furthermore, death plays a special role in Buddhist culture, and money collection after the passing of a village member has a long standing tradition in Cambodia. Finally, in the vignette situation the help goes to the family of the deceased and not to the person liable for the loss. These differences may explain the high proportion of providers who report to contribute the same amount in both situations. Nevertheless, we expect that those providers who reported lower contributions in case of irresponsible behavior in the survey to also reduce their transfers by more when recipients could have avoided their loss in the experiment.

We split the sample according to whether providers report in the survey to contribute less in Situation 2 than in Situation 1 (Column 2) or the same (Column 1) and analyze Specification

[19] Note that the survey was conducted two weeks before the experiment. It is therefore highly unlikely that the participants' behavior in the experiment was diluted by their response to the survey questions.

Table 3.8: Treatment Effect Analysis, by Survey Response

	(1) Transfer if $S1 = S2$	(2) Transfer if $S1 > S2$	(1) - (2) Effect Comparison
Inf (β)	-0.007	0.046	-0.053
	(0.129)	(0.144)	(0.271)
Opt (γ)	-0.528***	-0.908***	0.380*
	(0.099)	(0.244)	(0.234)
InfOpt (η)	-0.004	0.108	-0.112
	(0.088)	(0.170)	(0.197)
Constant (θ)	2.139***	2.292***	-0.153
	(0.125)	(0.257)	(0.283)
Observations	1068	260	
R2	0.030	0.067	
F	21.508	7.810	

Pooled OLS estimator; standard errors (wild cluster bootstrap) in parentheses.
(1) Participants who report in survey to contribute the same.
(2) Participants who report in survey to contribute less.
Transfers in 1,000 Riel.

3.2.[20] Table 3.8 reveals that providers that report in the survey to transfer less in case the outcome was affected by deliberate choice reduce their transfers significantly more (by 34%-40%) in response to foregoing insurance in the experiment than subjects who report to transfer the same amount (by 25%). We see this as an indication that the transfer behavior observed in the experiment is motivated by solidarity norms that are applied in real-life decision making.

Recipients' expectations

Those recipients that were informed about the transfer possibility (recipients of type $C1$ and $C2$) were asked to write down how much transfer they expected from the providers. We are interested in whether or not these beliefs change with the insurance option. If the recipients with the insurance option expected lower transfers from the providers than the recipients without the insurance option, the providers' behavior as illustrated above would be directly reflected in the recipients' beliefs.

Among all recipients who were informed, the average expected transfer amounts to 3,840 Riel without the option to insure and to 3,390 Riel with the option to insure (Column 1 of Table 3.9). The difference in the expected transfer of about 450 Riel is significantly larger than zero at the 5% level. Notably, this difference is close to the actual reduction in transfers by the providers when insurance becomes available – i.e., the roughly 600 Riel reduction in Table 3.4. This finding indicates that the recipients anticipated that providers condition their solidarity on insurance choice.

We now relate the average changes in providers' transfers in response to the insurance option to the average changes in recipients' beliefs at the village level. Figure 3.6 illustrates a strong

[20]The four participants, who reported in the survey to contribute more in Situation 2 than in Situation 1, are not considered.

Table 3.9: Expected Transfer by Recipients

	Expected Transfer
No Insurance Option	3.837
	(0.234)
Insurance Option	3.392
	(2.638)
Difference in Expected Transfers	0.446**
(between rounds)	(0.194)
Observations	166

Response to Belief Questions in Round 1 and Round 2; standard errors in parentheses
Transfers in 1,000 Riel

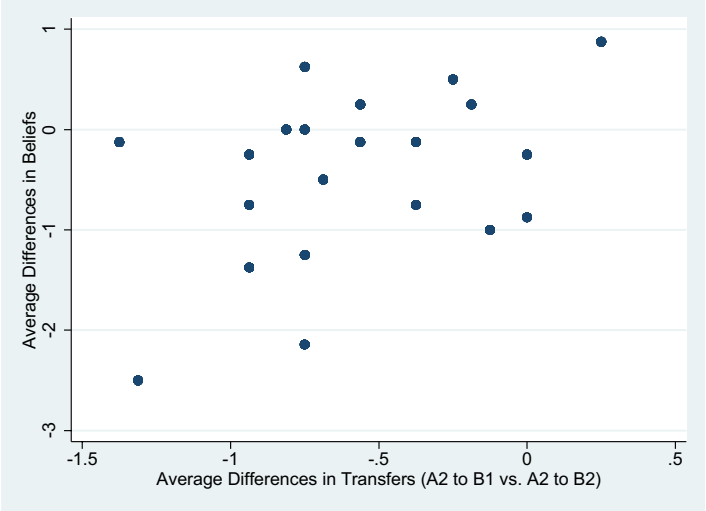


Figure 3.6: Differences in Beliefs and Transfers in Response to Insurance (Across Villages)

correlation between these two measures. The Pearson correlation coefficient is 0.41 with a p-value of 0.06. A simple regression without controls for village characteristics (results unreported) finds that the average change in transfers explains more than one sixth of the average change in beliefs. We argue that this correlation of providers' transfers and recipients' expectations at the village level is likely driven by the existence of village-level social norms on solidarity. Social norms, according to Bicchieri (2005), guide individuals' behavior through the expectation that others behave in line with the norm and also expect oneself to do so. If social solidarity norms, which generally guide villagers' behavior in real life, are here applied to the less familiar situation of the experiment, the correlation between providers' and recipients' behavior provides support for the external validity of our findings.

3.4.3.2 Underlying Preferences

Up to this point, we assumed the utility function in Equation 3.1 to represent the preferences of our experimental participants. However, a different specification of preferences can lead to similar findings. Let us assume that providers are motivated by social preferences that involve

both their own utility as well as the perceived utility of the recipient. Let us further assume that individuals differ in their risk preferences and that risk preferences tend to be personal information. Providers generally do not know the risk preferences and hence the utility of the recipients in the transfer game. If there was a selection of recipients into insurance based on risk preferences, recipients who choose not to take up insurance would signal their risk lovingness.[21] Providers may then act upon this signal and adjust their transfers accordingly. They would provide less transfers to recipients for whom they have reason to believe to be risk loving and thus not to suffer much from a loss. Given that this information becomes available only with the option of insurance, providers give less with the insurance option than without the insurance option. The reduction in transfers would then not be driven by the recipients' choice of foregoing insurance but by the signal that this choice provides about the recipients' utility curvature. We can provide some insight into the selection to insurance and the possibility of providers' acting on risk preferences in the following way.

We model the insurance uptake decision of the recipients in our experiment as a function of risk preferences as well as other characteristics that should determine insurance uptake according to the literature (Giné et al. 2008; Giesbert et al. 2011). For recipients that were uninformed about the transfer possibility, insurance uptake appears to be completely random (see Table B.5 in Appendix B.4.3). For informed recipients, risk preferences matter at a marginal level of significance. This indicates that risk preferences are unlikely to explain the insurance uptake per se. They rather seem to influence the recipients' willingness to rely on the providers' solidarity. Even so, the providers may nevertheless use the insurance uptake decision as a signal of risk preferences and respond accordingly.

Under the assumption that the knowledge of another person's risk preferences increases with the level of familiarity, we should expect that the signal in the experiment is of little value when providers transfer to people they know well. To gain insight into this idea, we can use information from the second game in our experiment in which the transfer game was played non-anonymously. If providers reacted solely to the risk preference rather than conditioning their transfers on choice, providers should be *less likely* to reduce their transfers in response to foregoing insurance, the *better* they know the recipient.

The second game in our experiment was similar to the first game with three main differences: 1) all subjects played both the role of the provider and the role of the recipient; 2) providers knew the name of the recipient they were matched with; recipients, in turn, were always informed that there was a provider but they did not know her identity; 3) the matched recipient for any provider remained the same for the treatment with insurance and for the treatment without insurance. Between the first and the second game, participants had to report for each of the other 31 participants per village whether they knew each other and, if so, whether they were relatives or close friends. In 9% of the matched pairs in the second game, the provider did not know the recipient; in 49% the provider knew the recipient but they were neither relatives nor friends; in 33% provider and recipient were related; and in 9% the provider reported to be a close

[21] Note that the price of the insurance was set relatively low, such that only risk loving people should forego the insurance.

friend with the recipient.

Table 3.10: Response to Foregoing Insurance in Game 2, by Relationship to Respondent

	Unknown	Known	Relative	Friend	Total
Reduction in Transfers	27.12	32.83	27.85	29.23	30.36
No Change	59.32	54.71	55.25	58.46	55.65
Increase in Transfers	13.56	12.46	16.89	12.31	13.99
Total	100.00	100.00	100.00	100.00	100.00

We analyze whether providers responded differently to the insurance uptake decision depending on how well they knew the recipient. We report the share of providers who transferred less, the same or more when insurance became available, separately for the different categories of acquaintance with the recipient. Table 3.10 illustrates that a comparable share of providers transferred less (the same, more) to the recipients in response to the insurance option across the categories. Most importantly, providers in the knowledge, blood relation and friendship categories are *not* less likely to reduce their transfers in response to the foregoing of insurance compared with providers in the first category (no knowledge). This implies that providers do not regard the insurance uptake decision solely as a signal of risk preferences. We do not mean to say that none of our experimental participants reacted on the perceived risk preference, but we can rule out that this is the only explanation for their behavior. They also or even dominantly respond to the choice of foregoing insurance per se.

3.5 Conclusion

In this study, we focus on informal transfers between peers which are motivated by social preferences. We investigate whether or not transfers to individuals who lost most of their income are reduced when these individuals could have avoided the loss by purchasing insurance. We formulate two hypotheses. First, individuals condition their solidarity on the choices of others (Hypothesis 1). And second, individuals condition their response to the choices of others on whether or not the others know that they might receive support (Hypothesis 2). Using a lab-in-the-field experiment in rural Cambodia, we obtain unequivocal support for Hypothesis 1 but not for Hypothesis 2. To our surprise, the providers' response to foregoing insurance do not depend on the level of information available to the recipients.[22] Possibly, individuals behave in line with Hypothesis 2 only if formal insurance is common. Familiarity with insurance may be a necessary condition to make people regard the reliance on others as free-riding. There is suggestive evidence for this idea in our experiment: providers who had purchased insurance in real life reduced their transfers by significantly more when insurance was foregone informedly

[22]This finding is in line with Koch and Normann (2008) and Dreber et al. (2013). These authors play dictator games, in which the recipients either know about the existence of a dictator who may transfer money to them or do not know about it. In both studies, the authors find that information on the side of the recipients is irrelevant for the dictators' transfer decisions. Dana et al. (2006) come to different results.

than providers who had no insurance in real life. As a follow-up, it would be desirable to think about an experimental design that varies the familiarity with insurance.

Most providers whom we interviewed after the experiment explained that they had made a positive transfer because they had felt pity for the recipient. The solidarity norms of our providers thus seem to be shaped by empathy.[23] Then, why do providers feel less empathy with a recipient who forewent insurance? As pointed out by Bowles (2008), economic incentives have a framing effect: they affect how a decision situation is represented and may imply appropriate behavior. The introduction of insurance may thus be interpreted as a signal that economic security becomes an individual responsibility, which reduces the moral obligation to support others (Landmann et al. 2012). This idea relates to findings by Hintz (2010) who analyzes the implementation of a life-insurance product in rural Indonesia. Hintz finds that in villages where the insurance was introduced, the willingness to provide help declined substantially after the insurance scheme was established. Hintz describes a paradigm shift: the insurance led to an "individualization of risk management ... (furthering) the erosion of social cohesion" (Hintz 2010, p. 232).

Our findings have potential implications for social preference driven support in developing countries. A non-negligible number of people support their ill or disabled relatives or friends without anticipating any transfers in return (De Weerdt and Fafchamps 2011). If people expect others to get insured when insurance becomes available, such support might be reduced. This would be particularly concerning if transfers were lowered to people who are too poor to pay for insurance premiums. Based on our experiment, in which people voluntarily forego insurance, it is impossible to evaluate the plausibility of this concern. Follow-up research would ideally vary the endowment of the recipients such that some people are able to pay for insurance premiums and others are not. If informal support turned out to be crowded out similarly to both groups of people, this would be a strong call for complementing insurance with social safety nets targeted at the poor.

[23] A number of studies suggest that empathy is one of the driving motives explaining pro-social behavior (e.g. see Andreoni and Rao (2011)).

Chapter 4

Conditional Solidarity and Informal Exchange

4.1 Introduction

In a lab-in-the-field experiment conducted in North-West Cambodia we elicited the extent to which people conditioned their support to another person on this person's prior choices (see Chapter 3). More specifically, we investigated whether villagers reduced their monetary transfers to a person in need if this person had the option to avoid her neediness by purchasing insurance. While on average we find a strong reduction in transfers, there is considerable heterogeneity in subjects' behavior. 44% of the subjects conditioned their support on the prior choice of the person in need – i.e., they reduced their transfers if the person in need could have avoided her neediness – yet 43% of the subjects transferred the same amount independent of the prior choice. How can this heterogeneity be explained?

There is considerable evidence both from surveys and experiments that many people consider whether a person is responsible for her outcome before they decide about the allocation of income, rewards or sanctions or when they make fairness judgments about redistribution policies (Schokkaert and Capeau 1991; Konow 2000; Cappelen et al. 2013a; Fong 2007; Jakiela 2015); that is, people evaluate the extent to which an outcome is the result of an individual's choice or of factors that lie outside of an individual's sphere of influence (Konow 2003). They allocate less to a person who put little effort in her work (Cappelen et al. 2007; Cappelen et al. 2013b; Jakiela 2015), who made risky investment decisions (Trhal and Radermacher 2009; Bolle and Costard 2015; Cappelen et al. 2013a), or who had the option to purchase insurance but forewent this option (Mollerstrom et al. 2015).

Notions of accountability differ systematically across countries and so does the extent to which individuals apply merit based fairness views when judging income re-distribution policies (Corneo and Grüner 2002; Alesina and Angeletos 2005; Reeskens and Oorschot 2013). But also within countries, individuals seem to differ in their understandings of fairness. While a large proportion of people allocate less to a person who put little effort in her work or made risky investment decisions, a non-negligible proportion allocate the same amount independent

of the recipient's effort. This has been shown to be the case for people living in high-income countries such as Germany, Norway and the U.S. (Konow 2000; Cappelen et al. 2007; Cappelen et al. 2013b; Almås et al. 2016), but also for lower-income countries such as Kenya, Uganda and Tanzania (Cappelen et al. 2013b; Jakiela 2015). There is limited evidence for the determinants of this heterogeneity. It is shown that part of the variation can be explained by differences in social upbringing: individuals coming from higher socioeconomic background, in terms of parental education and income, are more inclined to reward effort (Almås et al. 2016). Within a low-income country, individuals from economically more developed villages are shown to be more likely to reward effort (Jakiela 2015). Furthermore, there is some suggestive evidence that experience with joint production and resource allocation processes is positively correlated with the tendency to reward effort (Schäfer et al. 2015).

Holding others accountable for prior choices, by allocating income in proportion to a recipient's exerted effort or by transferring less when a recipient's neediness is self-inflicted, is, in the broader sense, a form of reciprocity. There is substantial experimental evidence that reciprocity can function as a social norm enforcement mechanism. Negative reciprocity (or the threat of it) can explain the high offers in ultimatum games (for an overview see Güth and Kocher (2014)), reduce free-riding and enhance cooperation in public-good games (Fehr and Gächter 2002; Ostrom 2006; Reuben and Riedl 2013) and increase the offered wage level in a gift-exchange game (Fehr et al. 1997; Fehr and Gächter 1998; Fehr and Gächter 2000). Reciprocity is thus a mechanism that can sustain arrangements between parties of competing interests when contracts are incomplete, and that can countervail the absence of a third party which could enforce contracts exogenously.[1] The question arises: do people who are more engaged in arrangements that rely on reciprocity as enforcement device also generally show a stronger tendency to hold others accountable?

In a context like rural Cambodia – where markets are less developed, most people work in the informal sector, access to financial services and insurance is limited and legal enforcement institutions are weak – people rely to a great extent on informal arrangements that are characterized by incomplete contracts and limited external enforcement possibilities. Neighbors and relatives transfer money in response to health shocks (Fafchamps and Gubert 2007b), yield is shared when crop-cycles are staggered (Platteau 2000) and people form labor sharing groups for the labor intensive harvest season (Krishnan and Sciubba 2009). Most of these arrangements are based on mutuality and are characterized by *exchange*; money, goods and services are exchanged against future compensation in kind (Kranton 1996). These mutual support arrangements need to be self-enforcing; to ensure compliance, the threat of punishment in case of defection must be sufficiently strong. When commitment is limited and the threat of punishment is weak, either because punishment is not severe enough or because the threat is not credible, the level of

[1] Similar to the studies on accountability, studies on negative reciprocity in public good, ultimatum and third party punishing games find substantial heterogeneity across and within countries in individuals' willingness to reciprocate (Henrich et al. 2005; Dohmen et al. 2008; Egas and Riedl 2008; Herrmann et al. 2008; Gächter and Herrmann 2009; Henrich et al. 2010; Falk et al. 2015). The observed differences can be related to differences in economic development and inequality at the country level, and to differences in age, gender and religion at the individual level.

support is sub-optimal or arrangements can break apart completely (Coate and Ravallion 1993; Foster and Rosenzweig 2001; Ligon et al. 2002). Thus, negative reciprocity, in the sense of holding exchange partners accountable when they are not behaving in the (implicitly) agreed-upon manner, is fundamental for the functioning of these informal arrangements.

In the present study, I investigate whether differences in engagement in informal exchange can explain part of the heterogeneity in the subjects' transfer decisions that we observe in our experiment; more specifically, whether subjects with more exposure to mutual support arrangements that are characterized by an exchange of money, goods and services show a stronger tendency to condition their support on prior choices and provide less support when neediness was self-inflicted.

I analyze this relationship, matching the experimental transfer data with data from a household survey conducted two weeks before the experiment. This survey elicited in detail different types of informal support arrangements between villagers, including exchange arrangements. I find that subjects with a larger 'exchange outdegree' – i.e., a larger number of informal exchange arrangements with other villagers – reduce their transfers by significantly more when neediness was self-inflicted. The correlation remains strong and significant when controlling for various subject characteristics that could affect the results, when excluding subjects with particularly large exchange networks and when using an exchange measure that is based on other villagers' reports, a subject's 'exchange indegree.' Furthermore, I show that the correlation is particularly strong the larger the number of exchange arrangements that take place outside the family. The results indicate that the tendency to hold others accountable and to condition transfers on prior choices is stronger among villagers that are more engaged in informal exchange arrangements.

The findings can be interpreted from two different angles. One way to interpret this is that engagement in informal exchange arrangements increases individuals' inclination to hold others accountable. They learn the importance of enforcing accountability for the stability of these arrangements, they internalize this behavior and act in accordance with it even in settings where the arrangement partner is unknown and no future interaction can be assumed (*learning argument*). On the other hand, one could argue that people's willingness to hold others accountable is not affected by their exposure to informal exchange; rather individuals with a stronger sense of accountability (due to innate characteristics or social upbringing) are more likely to select into informal exchange arrangements: as they pose a higher threat of punishment they face lower risk of a partner's non-compliance, the arrangements are more sustainable, and a higher level of exchange can be achieved (*selection argument*).

The finding is of particular relevance in the context of the recent expansion of formal insurance in developing countries. It is generally assumed that existing informal support networks can reduce the uptake of formal insurance in particular when they cover similar types of risks (Mobarak and Rosenzweig 2013; Dercon et al. 2014; Berg et al. 2017). In this study, however, I show that the willingness to sanction no-insurance uptake is particularly strong in villages with dense mutual support networks. If these higher sanctions are effective, then, ceteris paribus, insurance uptake in these villages might indeed be higher than in villages with less dense exchange networks. While certainly other factors also play a role and the net effect is not clear a priori,

the findings show that the relationship between formal and informal insurance is much more complex than often assumed.

The remainder of this chapter is structured as follows. In Section 4.2, I introduce the research setting in Cambodia. I discuss the importance of mutual support arrangements in rural Cambodia and describe the research project and the data that form the basis for the empirical analysis. In Section 4.3, I present the empirical method and the results of the analysis, and discuss their interpretation. Section 4.4 concludes.

4.2 Research Setting and Data

This study is based on data that were collected for a research project conducted in 21 villages in North-West Cambodia. The villages selected for the research are located along both sides of the river Stong-Sreng which separates the two provinces Banteay Meanchey and Siem Reap.[2] The focus of the research project was to analyze the interplay between formal insurance and informal support arrangements. The research consisted of two phases. First, in each village a household survey was conducted with 60 randomly selected households; and then, two weeks afterwards, in each village 32 of the respondents were invited to take part in an experiment.

4.2.1 Informal Exchange in Cambodia

In Cambodia, as in many other developing countries, most people have very limited access to formal financial products, that is credit, insurance or saving accounts, which could help them to better manage the risks they face such as health related emergencies or crop failure. While there is a high penetration of (mostly commercial) microfinance institutions (MFI), most offer loans for business investments; emergency loans are more seldom and typically associated with high costs.[3] Only around 4% of the adult population have savings at a financial institution and an even smaller proportion are covered by insurance (World Bank 2014; Microinsurance Network 2016). Informal support therefore plays a major role for coping with the consequences of shocks, but also for dealing with the daily changing circumstances, and villages are characterized by a complex system of mutual support arrangements (Murshid 1998; Kim 2001; Crochet 2011; Parsons 2016). These arrangements have a long-standing tradition in Cambodia and have survived the decades of turmoil that the country experienced.[4] The dominant forms of exchange are money, food and labor. In the rural areas most people are self-employed, e.g. as farmers, craftsmen or small scale vendors, thus the inflow of cash is highly variable. As a consequence, households borrow and

[2]Villages were selected to be comparable across the two provinces. Selection criteria included the size of the village, the level of migration and remoteness.

[3]According to the financial inclusion data provided by the World Bank, in 2013 nearly every third person above 15 had borrowed from a financial institution. But most loans are for investment purposes. The average loan size of MFIs that focus on poor clients has increased to 70% of median annual income in 2015 (MIMOSA 2016).

[4]Some scholars argue that the devastating era of the Khmer rouge regime and the following years of civil war, which did not come to an end before the mid 1990s, destroyed social cohesion and trust within villages and reduced inter-household exchange (Frings 1994; Ovesen et al. 1996). However, there is considerable evidence, including the data on hand, that informal exchange on village level have remained intact or at least regained its pivotal role (Murshid 1998; Kim 2001; Crochet 2011).

lend each other small amounts of money on a very frequent basis, typically without interest or a fixed repayment schedule. These *credit exchange arrangements* help the villagers to deal with income fluctuation (Parsons 2016). *Food exchange arrangements* serve a similar purpose. Many households grow rice, fruits and vegetables for their own consumption. Yet, harvest periods are staggered and yields vary, thus part of the harvest is often shared with other households, who have temporarily lower yields or harvest later; these households are generally expected to reciprocate this in the future (Murshid 1998; Kim 2001). *Provas dai*, a form of *labor exchange arrangement*, is another form of informal exchange that is very common in rural Cambodia. In this more institutionalized type of arrangement farmers self-organize in groups and help each other on a rotating basis in farming activities, in particular the harvest of rice (Kim 2001; Amakawa 2008).

In summary, the limited access to formal risk management options in rural Cambodia is met by a variety of exchange arrangements among the villagers. These informal arrangements are based on the, mostly implicit, agreement that provided support will be returned in the future.

4.2.2 Household Survey

The household survey elicited socioeconomic characteristics of all household members; access to formal risk management tools, such as insurance, savings and credit; and, most important for this research, information on the households' support networks within the villages, including credit, food as well as labor exchange arrangements. Each interview was conducted with the person responsible for the main decisions in the household, typically the household head, if available, or the spouse, and took on average two hours. In total 1,270 households were interviewed. At the end of each interview, the respondent was asked whether she would be willing and available to participate in a workshop (the experiment) that would take place two weeks after the survey had been conducted. From those respondents who answered affirmatively, in each village 32 individuals were invited to the experiment.[5] Care was taken to ensure that indeed the survey respondent participated in the experiment. In very few cases the respondent had to be replaced by another household member; in the analysis these cases are excluded.

4.2.2.1 Socioeconomic Characteristics of Provider Subjects

The analysis in this study focuses on those respondents who participated in the experiment, in particular those 336 subjects who played the role of the provider (in the following 'the provider subjects'). The experiment will be described in more detail below. Due to the selection procedure, the group of experiment participants are not representative for the overall population in the villages.

The socioeconomic characteristics of the provider subjects are presented in Table 4.1.[6] 68% of the subjects are female. More than two thirds are born in the village they reside. Subjects

[5]For the experiment, very old respondents were excluded and the focus was on the literate (for details on the selection method of the participants see Chapter 3). Furthermore, as the experiment took place during daytime, respondents who worked outside the village or had less flexible working schedules were less likely to take part.

[6]The provider subjects' socioeconomic characteristics differ only slightly from the characteristics of the pool of all survey respondents (see Table C.1 in Appendix C.1.1).

Table 4.1: Socioeconomic Characteristics of Provider Subjects

	mean	sd	min	max	median	count
Individual Characteristics						
Female	0.68	0.47	0	1	1	336
Age	38.90	12.05	18	77	36	336
Native	0.71	0.46	0	1	1	336
Household head	0.46	0.50	0	1	0	336
School years	2.95	3.07	0	14	2	336
Working	0.82	0.38	0	1	1	336
Working outside village	0.13	0.34	0	1	0	276
Selfemployed	0.86	0.35	0	1	1	276
Household Characteristics						
Household size	5.75	2.44	2	17	5	336
Female headed household	0.24	0.43	0	1	0	336
Total household income (USD)	160.91	399.83	0	5,000	50	299
Asset wealth	0.38	0.15	0	.92	.37	336
No electricity	0.72	0.45	0	1	1	336
Household grows rice	0.93	0.25	0	1	1	336
Farming as income source	0.80	0.40	0	1	1	336
Cultivated land (ha)	2.93	2.53	0	30	2.5	336
Loan at MFI	0.34	0.47	0	1	0	336
Bank account	0.04	0.20	0	1	0	336
Savings at fin. institution	0.03	0.17	0	1	0	336
Insurance	0.09	0.29	0	1	0	336
Observations	336					

went to school for on average of 3 years. 30% never went to school. Only 13% of the subjects, who are working, work outside the village.[7] 86% are self-employed – i.e., without stable cash inflow. Most of the subjects come from poor households. The average household income is 161 USD, however 50% have not earned more than 50 USD in the last month.[8] However, the income measure needs to be interpreted with care. 10% of the respondents refused to provide information for some or all income sources. Furthermore, income is typically fluctuating considerably over the year. As an alternative wealth measure, an asset index is derived.[9] 72% of the households have no electricity. Nearly all subjects' households grow rice for their own consumption, a large proportion also for sale. For four out of five households, revenues from farming is one source of income. On average, households cultivate three hectare of land; only one subject reported not to cultivate any land. Access to finance is very limited. While one third of the subjects reported to have borrowed from a financial institution (typically an MFI) in the last two years, only 4% have a bank account and even less hold savings at a financial institution. 9% of the households have insurance, in nearly all of the cases this is health insurance.

4.2.2.2 Informal Exchange

The villagers are engaged in a variety of support arrangements. In the household survey, most of these arrangements were elicited with the help of a social network questionnaire, where respondents were asked to identify other households within their village for different types of relationships. For each specific question, another household could only be named once; however, across the questions, households could be named several times. After respondents listed the names for each question, the interviewer asked them to identify the particular households, by showing a list of all households living within the village. This procedure allowed to match the households directly.

For the analysis in this study, I focus on informal exchange, which was reported for credit, food and labor. The arrangements are derived from the villagers' responses to the following questions:

1. If you suddenly needed to borrow 50,000 Riel for your household, who from this village do you typically ask?[10]
2. Who from this village would typically ask you for money (e.g. 50,000 Riel) for a sudden need and you would lend the money?
3. With whom within this village do you exchange in-kind goods on a regular basis?
4. If you grow rice, do you share labor with other people from this village (i.e., *Provas Dai*)?

[7] 18% of the subjects do not work regularly; most of them reported domestic work as their main activity.

[8] Income includes wage income, income from self-employment, state assistance, pension payments, rental income and remittances.

[9] The asset index is calculated by polychoric principal component analysis (Moser and Felton 2007). Variables are included based on their explanatory power. The following assets are included: ownership of tv, dvd player, smart phone, water pump, refrigerator, motorbike and handtractor; lifestock; roof material; source of lighting; general housing condition. The weights are derived based on the complete sample of surveyed households. The index is standardized to be between 0 and 1.

[10] 50,000 Riel are approximately 12 USD or three days of average wage for a factory worker (ILO 2015).

The first and second question are used as a proxy for the *credit exchange network*. In particular if a respondent names another household both as a borrowing as well as a lending link then this household is defined as being part of the credit exchange network of the respondent. The third question proxies the *food exchange network* and the fourth question the *labor exchange network* of the respondent. The traditional form of labor exchange in Cambodia is only among rice farmers and thus conditioned on growing rice. Households that do not grow rice (7% of the subjects' households) thus have no labor exchange arrangements.

The focus is explicitly on mutual support arrangements where support in terms of money, food or labor is expected from both sides. One-sided support arrangements are intentionally excluded, as the formation process differs (as shown in Chapter 2). While questions on food exchange and labor exchange ask explicitly for existing arrangements, the credit exchange arrangement is based on a hypothetical question. I assume that it measures an actual (and not a desired) link which will be activated when financial support is needed and is thus comparable to the other two exchange links.

In the latter analysis, information on social networks is included that are not specifically exchange related. This information is derived from the following questions:

5. In your free time whose house do you visit or who visits your house?
6. If you needed to make an important decision, who within this village would you turn to for advice?

The former question is used as a proxy for the *friendship network* and the latter for the *advice network* within the village.

For the analysis, the following notation is used. I define $g^{\ell}_{i,j;V} = 1$ if respondent i reports a link to j for network $\ell \in \mathcal{L} = \{$ *Friendship, Advice, CreditExchange, FoodExchange, LaborExchange*$\}$ and $g^{\ell}_{i,j;V} = 0$ otherwise, with $i \in N^S_V = \{1, ..., n^S_V\}$ and $j \in N_V = \{1, ..., n_V\}$, $i \neq j$, where N^S_V is the set of respondents – i.e., the set of surveyed households living in village V, and N_V is the set of all households living in village V, thus $N^S_V \subset N_V$. This information is used to calculate each respondent's outdegree for network ℓ. Respondent i's outdegree in network ℓ is defined as

$$\delta^{out}_{i,V}(\ell) = \sum_{j=1}^{n_V} g^{\ell}_{i,j;V}$$

and respondent i's overall exchange outdegree as

$$\delta^{out}_{i,V}(AnyExchange) = \delta^{out}_{i,V}(CreditExchange) + \delta^{out}_{i,V}(FoodExchange) + \delta^{out}_{i,V}(LaborExchange).$$

Table 4.2 describes the characteristics of the different network types for the provider subjects. The first column lists the average number of outgoing links, or the network density; the second column shows the proportion of subjects that named at least one other household for this specific network; the third column lists the average number of outgoing links for those subjects that reported at least one outgoing link; and the last two columns list the number of named households divided in related and unrelated households.

Table 4.2: Network Characteristics of Provider Subjects

Network ℓ	(1) Outdegree ($\delta^{out}_{i;V}(\ell)$)	(2) At least 1 link ($\delta^{out}_{i;V}(\ell) > 0$)	(3) Outdegree if $\delta^{out}_{i;V}(\ell) > 0$	(4) Links within family(**)	(5) Links outside family(**)
Friendship	3.61	0.99	3.65	2.23	1.38
	(1.89)	(0.09)	(1.87)	(1.85)	(1.58)
Advice	1.16	0.77	1.50	0.82	0.35
	(0.93)	(0.42)	(0.78)	(0.95)	(0.62)
CreditExchange	1.24	0.58	2.15	0.90	0.34
	(1.48)	(0.49)	(1.35)	(1.26)	(0.72)
FoodExchange	3.06	0.98	3.11	2.00	1.05
	(1.59)	(0.13)	(1.54)	1.58	1.22
LaborExchange(*)	2.29	0.55	4.19	1.36	0.93
	(2.84)	(0.50)	(2.61)	(2.00)	(1.71)
AnyExchange	6.43	0.99	6.47	2.57	1.59
	(4.44)	(0.08)	(4.43)	(2.04)	(1.91)
Observations	336				

Standard deviation in parentheses. (*) For households growing rice. (**) Links to distinct exchange partners.

Nearly all of the subjects identify at least one befriended household in their village. On average subjects listed four households as part of their friendship network. 77% of the subjects name at least one household they would turn to to ask for advice. 60% of the subjects are part of a credit exchange arrangement; on average they name two other households they would lend to or borrow money from. Nearly all subjects engage in food exchange arrangements, on average with three other households. 55% of the subjects that grow rice indicate to be part of a labor exchange arrangement with on average four other households. Overall, subjects have on average six exchange arrangements with four distinct exchange partners, of which around two thirds are family members.

Analyzing the main determinants for the size of the different networks of the survey respondents (see Table C.3 in Appendix C.1.2), we find that people who are born in the village as well as more educated people tend to have overall larger networks. Households with female household heads report on average less network partners; households that cultivate more land have greater credit, food and exchange networks, while smaller households tend to have larger advice networks.

It is important to note that the engagement in labor exchange arrangements varies substantially across villages. The proportion of respondents engaged in labor exchange ranges from below 20% to up to over 90%, while the proportion engaged in credit exchange and food exchange is more similar across villages (see Figure C.1 in Appendix C.1.2). This variation can partly be explained by the variation in average land size per household, which varies considerably across the villages.

4.2.3 Experiment

In the experiment, participants played two variations of a modified dictator game, which is called the transfer game. In this study, I focus only on one variation. For a more detailed description of the experiment, in particular the game design and the implementation in the field see Chapter 3.

4.2.3.1 Experimental Design

In each village the 32 participants were randomly assigned the role of provider or recipient. There were 16 providers and 16 recipients. Each provider (P) was anonymously matched with one recipient. Both received the same endowment, 16,000 Riel. Each recipient faced the probability of an income shock; with a 50% probability she could lose a large proportion of her endowment (14,000 Riel). Half of the recipients had no option to prevent this shock ($R1$) while the other half had the option to purchase an insurance for 6,000 Riel, which would cover the full loss from the shock ($R2$). Recipients were not aware that they were matched with providers.[11]

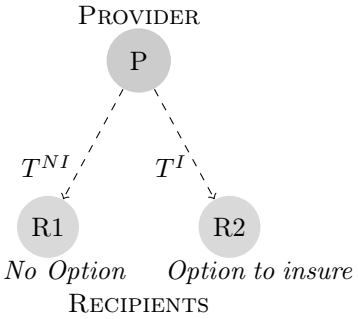


Figure 4.1: Transfer Game

The provider was explained the situation of the recipients and was asked to make two transfer decisions: how much of her endowment she would transfer if she was matched with a recipient of type $R1$ and the recipient had lost (T^{NI}), and how much she would transfer if she was matched with a recipient of type $R2$ and the recipient had lost (T^{I}). The basic design is depicted in Figure 4.1. The provider was aware that a recipient of group $R2$ would only lose if she had declined to purchase the insurance. The difference in the transfer decisions of each provider ($T^{I} - T^{NI}$) indicates the extent to which this subject conditions her support on the choice the recipient had to avoid her loss. It was hypothesized that the majority of subjects would condition their solidarity and transfer less if the recipient could be held accountable for her loss by not purchasing insurance.

4.2.3.2 Transfer Behavior in the Experiment

In the experiment, 336 subjects played the role of the provider. They made two transfer decisions, one for the case the recipient had the option to take up insurance (T^{I}) and one for the case where

[11] This is Case 2 of the transfer game described in Section 3.2.1.

the recipient did not have this option (T^{NI}). Figure 4.2 shows the distribution of transfers among the 336 subjects that played the provider role in the experiment. The figure shows a clear shift to zero transfers in case the recipient had the option to insure.

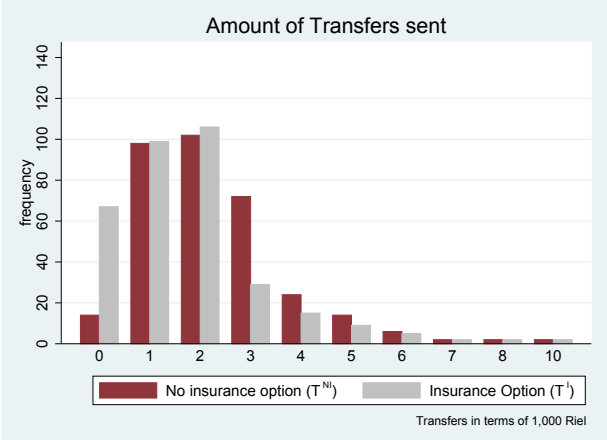


Figure 4.2: Transfer distribution

Indeed there is a significant reduction in the average transfers sent (see Table 4.3). If the recipient had no insurance option, providers transfer on average 2,200 Riel. However, in case the recipient had the option to insure but forewent this option, providers reduce their transfers by on average 600 Riel; a reduction of 28%. The results indicate that on average subjects condition their solidarity on choice and hold the recipient accountable for not taking up insurance. Yet, there

Table 4.3: Average Transfer

	No Insurance Option (T_{NI})	Insurance Option (T_I)	*Difference* *($T_I - T_{NI}$)*
Transfer in case of loss	2.155	1.577	*-0.598****
	(1.753)	(1.505)	*(1.534)*
Observations	336		

Transfers in terms of 1,000 Riel.

is considerable heterogeneity in transfer behavior (see Table 4.4). While on average transfers are significantly reduced when the insurance option is available, only 44% of the subjects actually do reduce their transfers in response to the recipient's choice; nearly the same proportion do not change their transfers at all. This does not seem to be primarily driven by a lack of understanding: when the sample is restricted to only those subjects that responded correctly to questions that were asked during the experiment to test the subjects' level of understanding, a similar pattern emerges.[12]

[12]Interestingly, 13% of the providers transfer *more* when recipients forewent the insurance. The underlying reasoning for this behavior is not directly apparent. A third of this is driven by erratic decisions of providers who did not clearly understand the experiment. However, for some subjects this decision was intentional. In

Table 4.4: Change in transfers

	Proportion of subjects	
	All	Test questions correct
No change ($T_{NI} = T_I$)	43.15%	44.57%
Reduction ($T_{NI} > T_I$)	44.05%	47.28 %
Increase ($T_{NI} < T_I$)	12.80%	8.15%
Observations	336	184

Change in transfers in response to recipient's insurance option.

In summary, the findings from the experiment suggest that a large proportion of the villagers take accountability principles into account in their transfer decisions even in an anonymous one-shot setting. However, many subjects do not react to the ex-ante choice of the recipient. The question is whether the transfer behavior can be related to the subjects' exposure to informal exchange outside the experiment. This will be the focus of the remainder of this chapter.

4.3 Empirical Analysis

4.3.1 Estimation Strategy

I investigate the relationship between the subjects' transfer behavior in the experiment and their engagement in informal exchange by estimating the following model

$$CS_{i,V} = \alpha + \beta Ex_{i,V} + \gamma X_{i,V} + \xi_V + u_{i,V}. \tag{4.1}$$

$CS_{i,V}$ describes the extent to which subject i living in village V conditions her support on whether the recipient did have the choice to avoid her loss. It is set in dependence to the subject's engagement in informal exchange $Ex_{i,V}$, controlling for individual characteristics $X_{i,V}$ and village fixed effects ξ_V.

I use two different specifications for $CS_{i,V}$. Both are based on the subject's transfer decisions in the experiment, $T^I_{i,V}$ and $T^{NI}_{i,V}$, where $T^I_{i,V}$ is the transfer of subject i if the recipient had the option to insure, and $T^{NI}_{i,V}$ is the transfer if the recipient did not have the option to insure, which I call the baseline transfer. In a first specification, $CS_{i,V}$ describes whether subject i reduced her transfer when the recipient had the option to insure. That is $CS_{i,V} = 1$ if $T^I_{i,V} < T^{NI}_{i,V}$ and $CS_{i,V} = 0$ else. I estimate the relationship between exposure to informal exchange and the likelihood of transfer reduction by logit regression with standard

interviews conducted with randomly selected subjects after the experiment, some stated they felt more pity with recipients who decided against the insurance and lost, than with recipients who just lost due to pure misfortune.

errors clustered on village level.[13] In a second specification, $CS_{i,V}$ describes the proportional change in transfers, that is $\frac{T^I_{i,V} - T^{NI}_{i,V}}{T^{NI}_{i,V}}$. The proportional rather than the absolute change is used, as the scope for changes in transfers directly depends on the size of the baseline transfer; using the absolute change as dependent variable can thus lead to spurious regression results.[14] I estimate the relationship between exposure to informal exchange and the proportional transfer change by OLS with standard errors clustered on village level. Using the proportional change as dependent variable, mechanically excludes all subjects for which $T^{NI} = 0$ (22 subjects). I assume $CS_{i,V}$ reflects a subject's preference to hold a person accountable for her choice. As it is arguable whether engagement in exchange predates the formation of such preference, β should be interpreted as a correlation not as a causal effect. I will discuss the interpretation in more detail in Section 4.3.3.

Ex_i describes the extent to which subject i engages in informal exchange. In the main analysis, Ex_i is measured by the number of exchange arrangements subject i reports; that is $\delta^{out}_{i,V}(ExchangeAny)$, in the following called the *Exchange network*.

A number of control variables, $X_{i,V}$, are included to account for factors that might affect both, engagement in informal exchange as well as the inclination to hold others accountable. To limit concerns of spurious regression I include three different sets of variables. First, I account for socio-demographic characteristics that can be assumed to be exogenous to the model. I include demographic characteristics such as gender and age ($Female$, Age, Age^2); whether the subject is born in the village ($Native$); the education ($School\ years$); and whether the subject is the household head ($Household\ head$). Second, social network characteristics are included that proxy a subject's sociability; subjects that are in general more social might be better in forming exchange arrangements, at the same time sociability can reflect a concern for the welfare of other people and thus possibly affects transfer decisions in the experiment. To control for sociability, I include the size of a subject's friendship network $\delta^{out}_{i,V}(Friendship)$ and the size of the advice network $\delta^{out}_{i,V}(Advice)$. A related concern is that subjects who are more engaged in informal exchange might have a larger social network within the village and thus on average closer links with other workshop participants. In the experiment, subjects do not know the identity of their transfer recipient, but they can form expectations based on the overall pool of participants; hence, the number of participants a subject is in close contact with might affect her transfer decisions. Since the pool of workshop participants was not randomly selected, this aspect is not necessarily captured by the friendship network. Therefore, I include the number of participants a subject identified as people she trusted (*Workshop network*).[15] Third, I control for economic characteristics that are not necessarily exogenous but could be correlated with the outcome and the explanatory variable of interest. To control for the general economic situation of a subject I

[13] When village fixed effects are included, bootstrap standard errors are used as recommended by Colin Cameron and Miller (2015).

[14] For comparison, the results for the main specification using the absolute transfer decisions T^{NI} and T^I as dependent variables are reported in Appendix C.2.2.

[15] This information is based on a network survey that was conducted with all workshop participants after the transfer game: each participant was asked to indicate for each other workshop participant whether she knew the person, whether they were relatives, whether they were friends and whether she trusted her.

include household's wealth measured by the asset index (*Asset wealth*); and whether a subject is self-employed (*Self-employed*) as the self-employed are presumably exposed to greater income fluctuations and are typically more dependent on their social support network; I include the size of land in hectare a subject's household cultivates (*Cultivated land*), which has been shown to affect the size of both food and credit exchange networks; and whether a subject grows rice (*Growing rice*), which is a precondition for labor exchange. Village fixed effects ξ_V are included to account for village specific variation.

Summary statistics for all variables included in the regression analysis are provided in Table C.2 in Appendix C.1.1. In three cases, the provider subject was not the original survey respondent but was replaced by another household member, as the original survey respondent was not available; in all of the following estimations these subjects are excluded.

4.3.2 Results

4.3.2.1 Conditional Solidarity and Informal Exchange

The control variables are subsequently added. In Tables 4.5 and 4.6, only the coefficients for the network variables are depicted; for the full tables see Tables C.4 and C.5 in Appendix C.2.1.

Table 4.5: Likelihood of Transfer Reduction and Engagement in Exchange

	(1)	(2)	(3)	(4)
	$P(T^I < T^{NI})$	$P(T^I < T^{NI})$	$P(T^I < T^{NI})$	$P(T^I < T^{NI})$
Exchange network	0.048*	0.068**	0.066**	0.063*
	(0.03)	(0.03)	(0.03)	(0.03)
Friendship network		-0.087	-0.090	-0.082
		(0.07)	(0.07)	(0.07)
Advice network		0.100	0.103	0.071
		(0.11)	(0.11)	(0.12)
Workshop network		-0.009	-0.011	-0.006
		(0.01)	(0.01)	(0.02)
Observations	333	333	333	333
Socio-demographic Controls	No	Yes	Yes	Yes
Economic Controls	No	No	Yes	Yes
Village fixed effects	No	No	No	Yes
Mean of dependent variable	0.441	0.441	0.441	0.441
r_p^2	0.008	0.020	0.022	0.022
χ^2	3.405	12.932	20.359	11.855
p	0.065	0.228	0.119	0.618

Estimated by logit. Standard errors in parentheses.
In Columns 1-3: standard errors clustered on village level; in Column 4: bootstrap standard errors.
Sociodemographic Controls: Female, Native, Age, Age^2, School years, Household head.
Economic Controls: Asset wealth, Selfemployed, Cultivated land (ha), Household grows rice.
Constant included but not reported.

We find a significant correlation between the size of a subject's exchange arrangement network and the likelihood that she reduces her transfer to a recipient who had the choice to insure against

Table 4.6: Change in Transfers and Engagement in Exchange

	(1) $\frac{T^I - T^{NI}}{T^{NI}}$	(2) $\frac{T^I - T^{NI}}{T^{NI}}$	(3) $\frac{T^I - T^{NI}}{T^{NI}}$	(4) $\frac{T^I - T^{NI}}{T^{NI}}$
Exchange network	-0.023**	-0.037***	-0.035***	-0.030***
	(0.01)	(0.01)	(0.01)	(0.01)
Friendship network		0.054*	0.056*	0.053*
		(0.03)	(0.03)	(0.03)
Advice network		-0.036	-0.046	-0.010
		(0.04)	(0.04)	(0.03)
Workshop network		0.012**	0.012***	0.011**
		(0.00)	(0.00)	(0.00)
Observations	311	311	311	311
Socio-demographic Controls	No	Yes	Yes	Yes
Economic Controls	No	No	Yes	Yes
Village fixed effects	No	No	No	Yes
Mean of dependent variable	-0.232	-0.232	-0.232	-0.232
r_a^2	0.025	0.064	0.066	0.040
F	7.662	2.698	2.420	2.613
p	0.012	0.028	0.035	0.025

Estimated by OLS. Standard errors in parentheses, clustered on village level.
Sociodemographic Controls: Female, Native, Age, Age^2, School years, Household head.
Economic Controls: Asset wealth, Selfemployed, Cultivated land (ha), Household grows rice.
Constant included but not reported.

the loss (Table 4.5). An increase in the exchange network by one standard deviation (4.44) increases the odds that a subject reduces her transfers by 24% ($e^{4.44 \cdot 0.048} = 1.24$). Similarly, the proportional change in transfers is significantly correlated with the size of the exchange network as depicted in Table 4.6. All else equal, an increase in the exchange network by one standard deviation is associated with a 10% point reduction in transfers ($4.44 \cdot -0.023 = -0.10$); this is nearly half of the average reduction of 23%. For both specifications, the correlation becomes more pronounced once socio-demographic and economic characteristics (Columns 2 – 4) and village fixed effects (Column 4) are included. While including village fixed effects improves the overall model's fit, individual level characteristics explain little of the observed variation (see Table C.5 in Appendix C.2.1). Subjects with a larger friendship network and subjects who trust a greater number of participants appear more lenient in their transfer reduction; while older subjects and subjects who are not born in the village tend to reduce their transfers by more.

We can also analyze the different types of exchange arrangements separately. Results are reported in Table C.6 in Appendix C.2.2. While each of the three types of exchange arrangements is significantly negatively correlated with the proportional change in transfers, the correlation is strongest for the case of credit and food exchange arrangements. One reason for the small predicted coefficient of labor exchange is the high variation of labor exchange arrangements across villages. Indeed when two villages where labor exchange is nearly non-existent are excluded from the analysis, the coefficient of labor exchange becomes larger and significant throughout

the specifications.

Overall, the results show a strong correlation between the size of a subject's exchange network and the extent she reduces her transfers when the recipient foregoes the insurance option.[16] Subjects who are more engaged in informal exchange show a stronger tendency to condition their support in the experiment. The results remain robust when controlling for various subject characteristics and village fixed effects.[17]

As a robustness check, I exclude subjects whose exchange network is more than two standard deviations above the average; this excludes subjects who reported more than 15 different exchange arrangements. Results are reported in Table C.8 in Appendix C.2.2. Columns 1 – 3 report the results for the likelihood of transfer reduction, Columns 4 – 6 report the results for the proportional change in transfers. For the estimation of the likelihood of transfer reduction, the estimated coefficient of $\delta^{out}_{i,V}(ExchangeAny)$ becomes insignificant.[18] The estimation of the proportional change $\frac{T^I - T^{NI}}{T^{NI}}$, on the other hand, remains robust and the estimated effect size is very similar to the previous estimation. This suggests that engagement in informal exchange can explain the degree of transfer reduction but less whether transfers are reduced at all. Part of this is likely due to the loss in information when aggregating the transfer behavior as a binary response variable. Henceforth, the focus will be on the proportional change.

4.3.2.2 A different network measure

One concern is that the reported size of the exchange network might not reflect the true network size; a subject's report is potentially biased by unobserved characteristics that also affect her transfer decisions (e.g. a wish to respond in line with the interviewer's or experimenter's expectations). This concern is more difficult to address; yet, the unique data structure allows deriving an additional indicator for the true size of a subject's exchange network by using the reports of the other survey respondents. In particular I derive a subject's indegree measured by the number of surveyed households that identified a subject's household as part of their social network. More specifically, for network $\ell \in \mathcal{L} = \{$*Friendship, Advice, CreditExchange, FoodExchange, LaborExchange*$\}$ subject i's indegree in network ℓ is defined as

$$\delta^{in}_{i,V}(\ell) = \sum_{j=1}^{n^S_V} g^{\ell}_{j,i;V},$$

[16] For comparison, Table C.7 in Appendix C.2.2 reports the estimation results for a subject's absolute transfer decisions.

[17] In addition to the reported control variables, various other variables have been tested: access to finance (e.g. having a bank account or insurance), household income and other occupational characteristics. Independent of these additional controls, the relationship between the exchange network and the reduction in transfers remains strong and significant.

[18] This is not driven by the restricted subject pool. If we use the same subject pool when estimating the likelihood of transfer reduction as we used for the estimation of the proportional change (i.e., excluding subjects for which $T^{NI} = 0$) results remain similar.

where $g^{\ell}_{j,i;V} = 1$ if respondent j mentions subject i's household as part of her network ℓ and 0 otherwise; with i and j both belonging to the pool of surveyed households – i.e., $i, j \in N^S_V = (1, .., n^S_V)$ where n^S_V is the number of surveyed households in village V. Subject i's overall exchange indegree is then defined as

$$\delta^{in}_{i,V}(AnyExchange) = \delta^{in}_{i,V}(CreditExchange) + \delta^{in}_{i,V}(FoodExchange) + \delta^{in}_{i,V}(LaborExchange).$$

I reestimate Model (4.1), including $\delta^{in}_{i,V}(Friendship)$, $\delta^{in}_{i,V}(Advice)$, $\delta^{in}_{i,V}(AnyExchange)$ instead

Table 4.7: Change in Transfers and Engagement in Exchange (measured by the exchange indegree)

	(1) $\frac{T^I - T^{NI}}{T^{NI}}$	(2) $\frac{T^I - T^{NI}}{T^{NI}}$	(3) $\frac{T^I - T^{NI}}{T^{NI}}$	(4) $\frac{T^I - T^{NI}}{T^{NI}}$
Exchange network (indegree)	-0.019*	-0.042**	-0.039*	-0.030*
	(0.01)	(0.02)	(0.02)	(0.02)
Friendship network (indegree)		0.036	0.031	0.026
		(0.03)	(0.03)	(0.03)
Advice network (indegree)		0.041	0.039	0.043
		(0.07)	(0.07)	(0.07)
Observations	311	311	311	311
Sociodemographic Controls	No	Yes	Yes	Yes
Economic Controls	No	No	Yes	Yes
Village fixed effects	No	No	No	Yes
Mean of dependent variable	-0.232	-0.232	-0.232	-0.232
r^2_a	0.010	0.025	0.022	0.007
F	3.093	3.016	3.495	5.582
p	0.094	0.019	0.006	0.000

Estimated by OLS. Standard errors in parentheses clustered on village level.
Sociodemographic Controls: Female, Native, Age, Age^2, School years, Household head.
Economic Controls: Asset wealth, Selfemployed, Cultivated land (ha), Household grows rice.
Constant included but not reported.

of the respective outdegree measures. Results are reported in Table 4.7. The previous set of control variables as well as village fixed effects are subsequently included. The coefficients of interest are less significant but they point into a similar direction as the outdegree measure. The larger a subject's exchange network in terms of incoming links the greater the average reduction in transfers in the experiment. The estimated effect size is similar to the outdegree measure. A one-standard deviation increase in the number of incoming exchange related links is associated with an additional 8% points reduction in transfers ($4.44 \cdot -0.019 = 0.084$) or, ceteris paribus, a 13% point reduction when controlling for socio-economic characteristics and village fixed effects. This provides some support that it is indeed the size of the exchange network that is driving the results and not (or, at least, to a lesser extent) unobserved respondent characteristics that affect the reported network size.

The indegree measure is not the perfect remedy. There are two short-comings. First, it likely underestimates a subject's true network size as not all households in the village were surveyed.

This would be less problematic, if the pool of surveyed households was truly random and thus representative for the village population. However, households could not be surveyed if no adult member was present during the survey period. Thus, the derived indegree likely underestimates the social network size to a larger extent for those subjects, who have proportionally more links to households whose adult members work outside the village or have temporarily migrated. When I regress the proportion of a subject's exchange network that was surveyed on the subject's socio-demographic and occupational characteristics as well as village fixed effects, none of the subject's characteristics except for age turn out to be significant.[19] This provides some indication that the proportion of a subject's exchange network that was surveyed is overall random or, at least, not systematically affected by characteristics that could also explain transfer decisions. Second, the indegree is strictly speaking the indegree of a subject's household and not of the subject herself; for each network question respondents were asked to identify the household they were linked to, not a specific household member. Yet, this potential upward bias of a subject's indegree is only problematic if it varies systematically across subjects and is correlated with characteristics which are not included in the regression that also affect transfer decisions. While this seems unlikely, the possibility cannot be ruled out.

4.3.2.3 Exchange within and outside the family

The exchange network measure used, $\delta^{out}_{i,V}(ExchangeAny)$, measures the sum of all exchange arrangements a subject i reports. However, a subject might have different types of exchange arrangements with the same partner. Furthermore, results might differ depending on whether exchange takes place within or outside the family. There are at least two reasons. First, with altruistic preferences a person is not only concerned about her personal welfare, but also about the welfare of her exchange partner, and thus her incentive to renege on an informally agreed upon arrangement is reduced (Foster and Rosenzweig 2001; Lin et al. 2014). Assuming altruism to be stronger among family members than among non-family members, a lower likelihood of defection implies a lower necessity for punishment in arrangements formed among family members. Second, even in the absence of altruistic preferences, defection is likely lower if an arrangement is formed within the family as the threat of social sanctions (i.e., sanctions by people who are not directly part of the arrangement) can be assumed to be stronger: the network among family members is likely more dense than the network outside the family and information sharing on defection more complete (Foster and Rosenzweig 2001); therefore more links are jeopardized in case of defection (Bloch et al. 2008). Hence, the social control through the family can reduce defection and thus the need for bilateral sanctions, which are, on the other side, of greater importance when an arrangement is formed outside the family. We could therefore expect that the inclination to hold others accountable is more pronounced for subjects that are more accustomed to exchange arrangements outside their family.

I repeat the estimation of Model 4.1 distinguishing between exchange within family and exchange outside the family, both for the number of exchange arrangements and for the number of

[19] On average, 50% of a subject's exchange partners were surveyed. The proportion reduces (on a declining rate) the older subjects are; yet, the effect is only marginally significant.

Table 4.8: Change in Transfers and Engagement in Exchange - within and outside the family

	(1) $\frac{T^I - T^{NI}}{T^{NI}}$	(2) $\frac{T^I - T^{NI}}{T^{NI}}$	(3) $\frac{T^I - T^{NI}}{T^{NI}}$	(4) $\frac{T^I - T^{NI}}{T^{NI}}$	(5) $\frac{T^I - T^{NI}}{T^{NI}}$	(6) $\frac{T^I - T^{NI}}{T^{NI}}$
Exchange network						
- within family	-0.019**	-0.033***	-0.027**			
	(0.01)	(0.01)	(0.01)			
- outside family	-0.035**	-0.041**	-0.038**			
	(0.01)	(0.02)	(0.02)			
Exchange partners						
- within family				-0.017	-0.042***	-0.024
				(0.01)	(0.01)	(0.02)
- outside family				-0.043*	-0.049**	-0.038*
				(0.02)	(0.02)	(0.02)
Observations	311	311	311	311	311	311
Control Variables	No	Yes	Yes	No	Yes	Yes
Village fixed effects	No	No	Yes	No	No	Yes
r_a^2	0.026	0.063	0.039	0.013	0.045	0.020
F	4.008	2.516	2.255	2.178	2.337	3.273
p	0.034	0.028	0.045	0.082	0.039	0.007

Estimated by OLS. Standard errors clustered on village level.
Control Variables: Friendship network, Advice network, Workshop network, Female, Native, Age, Age^2, Household head, School years, Asset wealth, Selfemployed, Cultivated land (ha), Household grows rice. Constant included but not reported.

exchange partners. Results are reported in Table 4.8. In Columns 1 – 3, as before, the number of exchange arrangements is used as main explanatory variable, yet now split in arrangements within and arrangements outside the family. In Columns 4 – 6, the number of distinct exchange partners is included instead; again distinguishing between family members and non-family members. First, we find that the estimation model that includes the number of exchange arrangements explains overall a larger proportion of the variation in transfer reduction than the model including the number of distinct exchange partners. Furthermore, we find that indeed the number of arrangements outside the family as well as the number of exchange partners outside the family are associated with a larger reduction in transfers than the number of exchange arrangements and exchange partners within the family. These findings are in line with the above theoretical reasoning. Subjects who engage more in exchange arrangements within their family reduce their transfers on average by less than subjects who are engaged in arrangements outside the family.

4.3.3 Discussion

A link between engagement in mutual support arrangements and the inclination to hold others accountable if neediness is self-inflicted does not seem unreasonable. In mutual support arrangements, people are expected to provide support when the arrangement partner is in need and to avoid excessive risk-taking that would increase her relative probability to be in need of support. Under limited commitment and with non-existing or imperfect external enforcement mechanisms,

the functioning of these arrangements crucially depends on the threat of punishment in case of non-compliance (Coate and Ravallion 1993; Foster and Rosenzweig 2001; Ligon et al. 2002); that is, it depends on an individual's readiness to hold the exchange partner accountable. While the observed correlation thus seems sensible, there are different ways to interpret the findings. An individual's capacity to hold others accountable might explain her ability to sustain a number of different informal exchange arrangements; on the other hand, it might be precisely the exposure to informal exchange that forms and enhances an individual's sense of accountability. The former interpretation argues with *selection*, the latter assumes a *learning* process. Each will be briefly discussed.

Holding others accountable for past choices and allocating less to someone who is responsible for her bad luck suggests an underlying fairness norm. According to the developmental psychology literature, notions of fairness are formed in childhood and adolescence. Children under the age of 6 already show a tendency to allocate more of a jointly produced good to those that contributed more to the good's production (Baumard et al. 2012; Liénard et al. 2013). This merit-based fairness notion becomes more pronounced during adolescence (Sigelman and Waitzman 1991; Kienbaum and Wilkening 2009; Meidenbauer et al. 2016). There is evidence for this also in the economic literature. Almås et al. (2010) show that the willingness to accept income inequalities as a result of differences in people's exerted effort significantly increases with age.[20] It is assumed that the perception of what is fair or unfair is affected by the social environment, the education at home and at school, and social interactions (Moore et al. 1993; Almås et al. 2010; Reeskens and Oorschot 2013; Almås et al. 2016; Schäfer et al. 2015). If notions of accountability are formed during childhood but are, by and large, stable thereafter, the findings from our experiment could be explained by a *selection process*. An individual with a more pronounced sense of accountability might be more inclined than others to select into informal exchange arrangements: she faces a lower risk of a partner's non-compliance due to the higher threat of punishment she poses, and thus higher levels of exchange can be achieved. Furthermore, as the sustainability of exchange arrangements depends on the level of punishment of non-compliance, individuals who hold others accountable are more likely to be part of an exchange arrangement that is sustained. Finally, these individuals might be more highly esteemed as exchange partners as they are assumed to show a greater willingness to account for their own actions and thus to be less likely to free-ride on other people's goodwill.

A different interpretation of the observed relationship is based on the literature on the evolution of fairness norms and cooperation (Chudek and Henrich 2011; Chudek et al. 2013; Boyd et al. 2003). These authors argue that norms of cooperation and the inclination to punish their non-adherence evolve as a strategy learned, adapted and imparted in order to sustain cooperation; it becomes internally motivated and part of an individual's preferences. There is some suggestive evidence that, in particular, experience with market exchange can affect how fairness rules are applied and how unfair behavior is punished. Analyzing fairness norms across small-

[20]The authors conducted a dictator game with a production stage with 5th – 13th graders in Norway. In their distribution decisions almost none of the 5th graders took the production achievements into account, while among the 13th graders meritocratism (i.e., allocating income in proportion to the exerted effort) was the dominant fairness rule applied.

scale societies, Henrich et al. (2010) find that in communities with stronger market integration (measured by the average proportion of calories purchased at a market) people are more likely to apply egalitarian fairness rules when asked to allocate a fixed amount of money between themselves and an anonymous partner and, furthermore, to punish unfair behavior. The authors argue that rules of cooperation were designed to sustain forms of market exchange, which frequently involve interactions with strangers and differ from the traditional mode of exchange; these rules resulted in local norms which were then also applied to other contexts. Findings by Jakiela (2015) can be related. Conducting a dictator game with production stage at a university in the U.S. and in different villages in Kenya, Jakiela finds that in the Kenyan sample villagers living in economically more developed villages (measured by the distance to the next paved road and to the electricity grid) tend to be more likely to reward effort.[21] An interpretation of our findings in line with this *learning argument* would suggest that individuals who are more exposed to informal exchange arrangements learn the importance of holding others accountable for the stability of these arrangements. They internalize this behavior and act in accordance also outside these arrangements, even in a setting where the partner is unknown no future interaction can be assumed.

To summarize, there are two potential interpretations for the observed correlation: one assumes selection, the other a learning process. With the data on hand, I cannot determine which interpretation captures the underlying mechanisms more accurately. Indeed, it could be a combination of both. People growing up in an environment that is more favorable to mutual support arrangements, learn the importance of accountability principles early on; when they engage in informal exchange arrangements themselves this notion is strengthened; in particular, when they deal with people outside their own family. Some individuals might be better than others in following and enforcing the rules on which these arrangements are built upon due to innate disposition or upbringing, and are thus able to maintain a higher number of different exchange arrangements than others. But this is only a speculation; more in-depth research is needed to understand the underlying mechanisms.

4.4 Conclusion

In this chapter, I show that an individual's inclination to hold someone accountable for prior choices is correlated with the extent to which she engages in mutual support arrangements, in particular the informal exchange of money, food and labor. Individuals with larger exchange networks reduce their support to a person, who could have avoided her neediness, by more. The correlation is particularly strong for exchange arrangements formed outside the family. Furthermore, the findings suggest that primarily the number of arrangements an individual maintains matter and, to a lesser extent, the number of distinct exchange partners.

There are a number of limitations to the study that need to be considered before discussing the policy implications of the findings.

[21] Jakiela provides a slightly different interpretation of her findings. She argues that economic development increases people's respect for others' earned property rights.

First, I can only show a correlation. I cannot identify a causal link, and there are several plausible interpretations of the findings.

Second, the sample is very specific: the area, where the research is conducted is very poor and has been heavily affected by the civil war following the Pol Pot regime (Chandler 2007; ADB 2014); the surveyed villages were not randomly chosen but selected to be rather small and not too close to the next urban center; finally, as discussed above there has been a selection of subjects into the experiment, and people who worked outside the village were less likely to partake. All these factors likely increase the size of the observed inter-household exchange networks. Furthermore, the focus was explicitly on exchange arrangements within the village; arrangements with people outside the village are neglected.

Third, the true number of an individual's exchange arrangements is unobserved; I construct the measure based on the individual's reports. If misreporting is correlated with notions of accountability this might affect the results. Showing that the results hold when using the indegree measure reduces these concerns; however, I cannot rule them out completely.

Finally, I interpret the subjects' transfer decisions as indication for their individual fairness perceptions. But other motives might have affected the transfer decisions. For example, provider subjects might have made their transfer decisions contingent on the expected preferences and character traits of the recipient; they then might have taken the choice of the recipient as a signal of her preferences (e.g. her risk preferences) and adjusted their transfers accordingly.[22] A related aspect is the role of presumed intentions. It has been shown that perceptions of intentions vary across individuals and societies (Barrett et al. 2016). It might be the case that the observed differences in punishment behavior in the experiment are not solely driven by differences in the willingness to hold the recipient accountable for her prior choice but also by the presumed intentions that affected the recipient's choice. Indeed, it is shown how an individual interprets certain behavior and the underlying intentions is affected by her social upbringing and experience with social interaction (Dunn et al. 2000; Arsenio and Gold 2006; Vera-Estay et al. 2016); it thus might also be correlated with exposure to informal exchange. As we have not elicited perceived intention this potential link cannot be analyzed in more detail. Further research is needed.

Notwithstanding these limitations, the findings of this study are of particular policy relevance. In recent years there have been considerable efforts to expand formal insurance in developing countries. This aroused a debate on the interplay of formal insurance and the informal mutual support arrangements. While some scholars are worried that formal insurance will crowd out the traditional informal insurance arrangememts (Hintz 2010; Landmann et al. 2012, others argue that a strong network of informal insurance will rather result in a lower uptake of formal insurance (Mobarak and Rosenzweig 2013; Dercon et al. 2014; Berg et al. 2017). Part of the overall effect depends on how no-insurance uptake is received by other villagers. If there is a general tendency to sanction villagers who do not take up insurance – i.e., to reduce the support to those who could have avoided their neediness – then the pressure to take up insurance is high. In this chapter, I show that this pressure is high even in villages with dense networks of informal

[22] In Chapter 3, we argue that this is unlikely to be the dominant motive, but we cannot rule out that some subjects act in this manner.

exchange arrangements; in fact, I show that in these villages the tendency to sanction irresponsible behavior is even higher; particularly so, if these exchange arrangements take place outside the family network. Ceteris paribus, the stronger threat of sanctions in villages with dense exchange arrangements could result in a relatively higher insurance uptake. These findings suggest that the link between informal support arrangements and formal insurance is very complex. In order to make predictions on the impact of formal insurance on informal institutions, both the structure of existing support arrangements as well as the mechanisms that govern the behavior within these arrangements need to be considered.

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Appendix A

Appendix for Chapter 2

A.1 Descriptives

A.1.1 Summary Statistics

Table A.1: Household Characteristics (all villages)

	mean	sd	min	max	median	count
Household size	4.14	2.01	1	11	4	359
Female head	0.34	0.47	0	1	0	359
Head has no basic education	0.24	0.43	0	1	0	358
Head completed high school	0.35	0.48	0	1	0	358
No. of family hh within village	10.77	7.15	0	40	10	359
No. of family hh outside village	3.30	3.99	0	50	3	359
% of adults working	0.53	0.35	0	1.5	.5	357
% of adults working outside village	0.08	0.19	0	1	0	357
Covered by social security	0.27	0.44	0	1	0	359
Fishing as main income source	0.20	0.40	0	1	0	294
Farming as main income source	0.19	0.39	0	1	0	294
HH income last month (PHP)	13,225.56	31,225.09	0	330,975	5,800	359
Asset wealth	0.36	0.21	0	1	.33	358
OFW exists	0.13	0.33	0	1	0	357
Remittances recipient	0.52	0.50	0	1	1	359
Amount remittances last year (PHP)	34,228.40	47,443.59	100	312,000	12,300	187
Coop member	0.13	0.34	0	1	0	359
Bank account	0.06	0.24	0	1	0	359
MFI member	0.16	0.37	0	1	0	359
Health insurance	0.53	0.50	0	1	1	359
Informal borrowing and lending	0.63	0.43	0	1	1	359
Observations	359					

Surveyed 359 households from 22 villages, covering 1485 household members; hh - households; PHP - Philippine Pesos.
*) Income from last month; includes salary, proceeds from self-employment, remittances, loans, public assistance, pensions, payouts from savings and other income (such as gambling).

Table A.2: Summary Statistics of Variables used in Neediness Analysis

	mean	sd	min	max	median	count
Age of head	53.84	15.72	4	98	53	306
Female head	0.33	0.47	0	1	0	306
Head completed high school	0.33	0.47	0	1	0	306
Household size	4.25	1.98	1	11	4	306
Asset wealth	0.36	0.21	0	.99	.33	306
No. links outside village	1.99	1.73	0	9	2	306
Remittances recipient	0.63	0.48	0	1	1	306
Durable asset wealth	0.42	0.20	0	.99	.41	306
No. of family hh outside village	3.42	4.21	0	50	3	306
Access to credit	0.52	0.50	0	1	1	306
Health insurance	0.52	0.50	0	1	1	306
Observations	306					

Based on the sample of 306 surveyed households in the 22 villages, including a random sample of 14 households from Maramig.

Table A.3: Summary Statistics of Variables used in Dyadic Regressions

	mean	sd	min	max	median	count
Sum in Asset wealth	0.77	0.28	.049	1.8	.77	2080
Sum in Household size	6.92	2.39	2	16	7	2080
Sum in % hh members (16-59)	1.00	0.43	0	2	1	2080
Sum in No. of family hh within village	18.92	9.07	0	49	18	2080
Sum in No. of family hh outside village	6.34	3.94	0	25	6	2080
Sum in Age of head	110.89	22.84	51	176	111	2080
Sum in Head completed high school	0.80	0.69	0	2	1	2080
Sum in Female head	0.71	0.67	0	2	1	2080
Sum in Health insurance	1.23	0.68	0	2	1	2080
Sum in Access to credit	0.89	0.70	0	2	1	2080
Sum in Receives remittances	1.32	0.66	0	2	1	2080
Sum in $\widehat{needy}$	0.68	0.36	.0049	2	.6	2080
AbsDiff in Asset wealth	0.23	0.17	0	1	.2	2080
AbsDiff in Household size	1.91	1.49	0	7	2	2080
AbsDiff in % hh members (16-59)	0.35	0.26	0	1	.33	2080
AbsDiff in No. of family hh within village	7.35	5.56	0	25	6	2080
AbsDiff in No. of family hh outside village	3.01	2.65	0	15	2	2080
AbsDiff in Age of head	18.98	13.33	0	66	17	2080
AbsDiff in Head completed high school	0.49	0.50	0	1	0	2080
AbsDiff in Female head	0.46	0.50	0	1	0	2080
AbsDiff in Health insurance	0.48	0.50	0	1	0	2080
AbsDiff in Access to credit	0.50	0.50	0	1	1	2080
AbsDiff in Receives remittances	0.45	0.50	0	1	0	2080
AbsDiff in $\widehat{needy}$	0.28	0.24	.00021	1	.22	2080
commonfriend	0.35	0.48	0	1	0	2080
Observations	2080					

Based on the 2080 undirected links. hh - household.

A.1.2 Asset Wealth

Derivation of Asset Index

I derive two asset indices: an index on general asset wealth (the variable *Asset wealth*) and an index on durable asset wealth (the variable *Durable asset wealth*). These indices are derived, using polychoric principal component analysis (Moser and Felton 2007), on the basis of the larger data set of the 22 villages. Variables are chosen based on the proportion of variance they explain. For the summary statistics of all included variables for both indices see Table A.4. The weights are assigned using the first component. Each index is standardized to be between 0 and 1.

Table A.4: Assets included in Asset Indices

	mean	sd	min	max	p50	count
land	0.36	0.89	0	4	0	358
cooking	1.23	0.61	1	5	1	358
lighting	4.74	0.60	1	5	5	358
water	3.57	1.26	2	6	4	358
walls	2.70	0.84	1	4	3	358
roof	3.21	1.85	1	5	5	358
floor	3.37	1.11	1	5	4	358
toilet	2.91	0.63	1	4	3	358
fridge	0.25	0.43	0	1	0	358
tv	0.55	0.50	0	1	1	358
ricecook	0.15	0.35	0	1	0	358
dvd	0.33	0.47	0	1	0	358
radio	0.50	0.50	0	1	1	358
pc	0.05	0.22	0	1	0	358
Observations	358					

Based on 358 households from 22 villages.

The general asset wealth index includes variables that describe assets and housing characteristics. The following variables are included: land size; wall-, roof- and floor material of the house; source of energy for cooking and lighting; source of water; type of toilet; furthermore, an indicator for whether the household owns one of the following assets: ricecooker, fridge, tv, washing machine, dvd recorder, radio and PC. The outcome of the component analysis is reported in Table A.5.

The durable asset wealth index includes the following variables: land size; wall-, roof- and floor material of the house; source of energy for cooking and lighting; source of water; and the type of toilet (see Table A.6). The outcome of the component analysis is reported in Table A.6.

Internal Validity

For an index to be internally valid, the correlation among the variables included should be sufficiently high. This is the case both for the general asset wealth (Table A.7) as well as for the durable asset wealth (Table A.8).

Table A.5: General Asset Wealth - Principal Components

k	Eigenvalues	Proportion explained	Cumulative proportion explained
1	7.02	0.50	0.50
2	1.31	0.09	0.59
3	1.04	0.07	0.67
4	0.92	0.07	0.74
5	0.71	0.05	0.79
6	0.68	0.05	0.83
7	0.54	0.04	0.87
8	0.53	0.04	0.91
9	0.36	0.03	0.94
10	0.31	0.02	0.96
11	0.23	0.02	0.98
12	0.17	0.01	0.99
13	0.15	0.01	1.00
14	0.03	0.00	1.00

Polychoric principal component analysis;

Based on asset ownership of 358 households from 22 villages.

Table A.6: Durable Asset Wealth - Principal Components

k	Eigenvalues	Proportion explained	Cumulative proportion explained
1	3.62	0.45	0.45
2	1.20	0.15	0.60
3	0.84	0.10	0.71
4	0.74	0.09	0.80
5	0.58	0.07	0.87
6	0.47	0.06	0.93
7	0.38	0.05	0.98
8	0.17	0.02	1.00

Polychoric principal component analysis.

Based on asset ownership of 358 households from 22 villages.

Table A.7: Correlation among Assets for General Asset Wealth Index

	land	cooking	lighting	water	walls	roof	floor	toilet	fridge	tv	ricecook	dvd	radio
land	1.00												
cooking	0.06	1.00											
	0.24												
lighting	0.11	0.03	1.00										
	0.04	0.58											
water	0.05	0.17	-0.05	1.00									
	0.30	0.00	0.35										
walls	0.20	0.25	0.17	0.15	1.00								
	0.00	0.00	0.00	0.00									
roof	0.27	0.22	0.17	0.16	0.61	1.00							
	0.00	0.00	0.00	0.00	0.00								
floor	0.20	0.18	0.23	0.10	0.53	0.40	1.00						
	0.00	0.00	0.00	0.05	0.00	0.00							
toilet	0.08	0.21	0.16	0.22	0.33	0.21	0.26	1.00					
	0.15	0.00	0.00	0.00	0.00	0.00	0.00						
fridge	0.25	0.32	0.14	0.12	0.46	0.40	0.39	0.27	1.00				
	0.00	0.00	0.01	0.03	0.00	0.00	0.00	0.00					
tv	0.18	0.21	0.30	0.12	0.39	0.44	0.36	0.25	0.44	1.00			
	0.00	0.00	0.00	0.02	0.00	0.00	0.00	0.00	0.00				
ricecook	0.18	0.34	0.12	0.14	0.31	0.27	0.25	0.24	0.47	0.35	1.00		
	0.00	0.00	0.02	0.01	0.00	0.00	0.00	0.00	0.00	0.00			
dvd	0.19	0.21	0.20	0.12	0.40	0.34	0.35	0.23	0.43	0.58	0.42	1.00	
	0.00	0.00	0.00	0.02	0.00	0.00	0.00	0.00	0.00	0.00	0.00		
radio	0.23	0.09	0.11	0.16	0.24	0.29	0.17	0.20	0.22	0.26	0.22	0.22	1.00
	0.00	0.11	0.03	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	
pc	0.14	0.26	0.02	0.15	0.23	0.16	0.20	0.21	0.29	0.16	0.36	0.26	0.16
	0.01	0.00	0.73	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Based on 358 households from 22 villages; p-values in second line of each row

Table A.8: Correlation among Assets for Durable Asset Wealth Index

	land	cooking	lighting	water	walls	roof	floor
land	1.00						
cooking	0.06	1.00					
	0.24						
lighting	0.11	0.03	1.00				
	0.04	0.58					
water	0.05	0.17	-0.05	1.00			
	0.30	0.00	0.35				
walls	0.20	0.25	0.17	0.15	1.00		
	0.00	0.00	0.00	0.00			
roof	0.27	0.22	0.17	0.16	0.61	1.00	
	0.00	0.00	0.00	0.00	0.00		
floor	0.20	0.18	0.23	0.10	0.53	0.40	1.00
	0.00	0.00	0.00	0.05	0.00	0.00	
toilet	0.08	0.21	0.16	0.22	0.33	0.21	0.26
	0.15	0.00	0.00	0.00	0.00	0.00	0.00

Based on 358 households from 22 villages; p-values in second line of each row.

External Validity

To test the external validity of the derived indices, we compare the distribution of each asset index with the income distribution (Table A.9 for the general asset wealth index and Table A.11 for the durable asset wealth index) as well as with the distribution of a measure of subjective wealth (Table A.10 for the general asset wealth index and Table A.12 for the durable asset wealth index). The subjective wealth measure is based on the question *'On a scale from 1 to 10 how would you describe your household's wealth status compared to the other households in the barangay? (1 - much less wealthy; 10 - much more wealthy).*' The distributions are, by and large, similar which supports the external validity of the indices.

Table A.9: Quartile Comparison: General Asset Wealth Index vs. Household Income

	Quartiles of Household Income Distribution				
	1	2	3	4	Total
1	35.56	30.00	22.22	12.22	100.00
2	34.83	26.97	23.60	14.61	100.00
3	20.00	28.89	26.67	24.44	100.00
4	12.36	11.24	28.09	48.31	100.00
Total	25.70	24.30	25.14	24.86	100.00
Observations	358				

Based on 358 households from 22 villages.

Table A.10: Quartile Comparison: General Asset Wealth Index vs. Selfreported Wellbeing

	Quartiles of Selfreported Wellbeing Distribution				
	1	2	3	4	Total
1	64.44	30.00	5.56	0.00	100.00
2	46.07	32.58	19.10	2.25	100.00
3	30.00	24.44	38.89	6.67	100.00
4	5.62	7.87	48.31	38.20	100.00
Total	36.59	23.74	27.93	11.73	100.00
Observations	358				

Based on 358 households from 22 villages.

Table A.11: Quartile Comparison: Durable Asset Wealth Index vs. Household Income

	Quartiles of Household Income Distribution				
	1	2	3	4	Total
1	36.96	30.43	20.65	11.96	100.00
2	31.03	31.03	24.14	13.79	100.00
3	17.78	22.22	32.22	27.78	100.00
4	16.85	13.48	23.60	46.07	100.00
Total	25.70	24.30	25.14	24.86	100.00
Observations	358				

Based on 358 households from 22 villages.

Table A.12: Quartile Comparison: Durable Asset Wealth Index vs. Selfreported Wellbeing

	Quartiles of Selfreported Wellbeing Distribution				
	1	2	3	4	Total
1	60.87	30.43	6.52	2.17	100.00
2	44.83	34.48	18.39	2.30	100.00
3	25.56	21.11	42.22	11.11	100.00
4	14.61	8.99	44.94	31.46	100.00
Total	36.59	23.74	27.93	11.73	100.00
Observations	358				

Based on 358 households from 22 villages.

A.2 Data Analysis

A.2.1 Healthshocks and Coping Strategies

Table A.13: Determinants of Health Shock in the Past (all villages)

	(1) Health Shock
Female head	-0.266
	(0.284)
Head completed high school	0.280
	(0.306)
No. hh members (<6)	0.405**
	(0.181)
No. hh members (6-15)	-0.167
	(0.130)
No. hh members (16-49)	0.059
	(0.109)
No. hh members (50-69)	-0.134
	(0.187)
No. hh members (>70)	0.101
	(0.269)
Asset wealth	0.861
	(0.776)
Main work in public sector	0.162
	(0.575)
Main work in agriculture	0.262
	(0.379)
Main work in trade or craftmenship	-0.045
	(0.495)
Main work in transport or construction	-0.032
	(0.517)
Main work in services	-0.287
	(0.576)
Constant	-0.965
	(0.819)
Village fixed effects	Yes
Observations	306
Mean of Dependent Variable	0.484
log likelihood	-190.630
χ^2	42.620
p	0.147
r_p^2	0.101

Logit estimation. Standard errors clustered on village level in parentheses
Based on the sample of the 306 surveyed households in the 22 villages, including a random sample of 14 households from Maramig.

Table A.14: Actual Coping Strategy (all villages)

	freq	pct
Loan from informal group/moneylender	6	1.58
Loan from formal institution (bank, mfi)	5	1.32
Use of savings	6	1.58
Use benefits from insurance	43	11.32
Sale of assets	14	3.68
State assistance, assistance from community	14	3.68
Work more	4	1.05
Reduce food consumption	1	0.26
Nothing	40	10.53
Other specify	1	0.26
Loan from SSS, GSIS, Pag-ibig	3	0.79
Loan from association, coop	1	0.26
Monetary gift from relatives - within Barangay	57	15.00
Monetary gift from relatives - outside Barangay	105	27.63
Monetary gift from friends, neighbors - within Barangay	4	1.05
Monetary gift from friends - outside Barangay	7	1.84
Nonfinancial help by relatives - within barangay	35	9.21
Nonfinancial help by relatives - outside barangay	2	0.53
Nonfinancial help by friends, neighbors - within barangay	7	1.84
Nonfinancial help by friends - outside barangay	0	0.00
Loan from relatives - within Barangay - with interest	0	0.00
Loan from relatives - within Barangay - without interest	10	2.63
Loan from relatives - outside Barangay - with interest	1	0.26
Loan from relatives - outside Barangay - without interest	1	0.26
Loan from friends/neighbors - within Barangay - interest	3	0.79
Loan from friends/neighbors - within Barangay - no interest	7	1.84
Loan from friends - outside Barangay - interest	0	0.00
Loan from friends - outside Barangay - no interest	3	0.79
Total	380	100.00
Observations	380	

Coping strategies listed in response to a health emergency in past 3 years.
Responses of the 148 households who experienced a health emergency.
Based on the sample of 306 surveyed households in the 22 villages, including a random sample of 14 households from Maramig.

Table A.15: Hypothetical Coping Strategy (all villages)

	freq	pct
Loan from informal group/moneylender	5	0.56
Loan from formal institution (bank, mfi)	26	2.91
Use of savings	9	1.01
Use benefits from insurance	97	10.84
Sale of assets	52	5.81
State assistance, assistance from community	61	6.82
Assistance by ngo	3	0.34
Work more	12	1.34
Nothing	42	4.69
Other specify	2	0.22
Loan from SSS, GSIS, Pag-ibig	27	3.02
Loan from association, coop	4	0.45
Monetary gift from relatives - within Barangay	148	16.54
Monetary gift from relatives - outside Barangay	220	24.58
Monetary gift from friends, neighbors - within Barangay	23	2.57
Monetary gift from friends - outside Barangay	11	1.23
Nonfinancial help by relatives - within barangay	58	6.48
Nonfinancial help by relatives - outside barangay	5	0.56
Nonfinancial help by friends, neighbors - within barangay	9	1.01
Nonfinancial help by friends - outside barangay	1	0.11
Loan from relatives - within Barangay - with interest	2	0.22
Loan from relatives - within Barangay - without interest	35	3.91
Loan from relatives - outside Barangay - with interest	2	0.22
Loan from relatives - outside Barangay - without interest	8	0.89
Loan from friends/neighbors - within Barangay - interest	2	0.22
Loan from friends/neighbors - within Barangay - no interest	21	2.35
Loan from friends - outside Barangay - interest	0	0.00
Loan from friends - outside Barangay - no interest	10	1.12
Total	895	100.00
Observations	895	

Coping strategies listed in response to a hypothetical health emergency.
Based on the sample of 306 surveyed households in the 22 villages, including a random sample of 14 households from Maramig.

A.2.2 Dyadic Analysis - Full Tables

Table A.16: Likelihood of Support Link, full table

	(1) $P(s_{ij}=1)$	(2) $P(s_{ij}=1)$
Asset wealth	-0.715	
	(0.671)	
Household size	0.047	
	(0.057)	
% hh members (16-59)	0.104	
	(0.231)	
No. of family hh within village	0.031**	
	(0.013)	
No. of family hh outside village	-0.054*	
	(0.030)	
Age of head	-0.013**	
	(0.007)	
Head completed high school	-0.332**	
	(0.149)	
Female head	-0.111	
	(0.213)	
Alter: Asset wealth	1.790***	
	(0.685)	
Alter: Household size	0.129**	
	(0.062)	
Alter: % hh members (16-59)	0.046	
	(0.334)	
Alter: No. of family hh within village	0.000	
	(0.014)	
Alter: No. of family hh outside village	-0.056*	
	(0.032)	
Alter: Age of head	-0.001	
	(0.009)	
Alter: Head completed high school	-0.093	
	(0.163)	
Alter: Female head	0.058	
	(0.214)	
AbsDiff in Asset wealth	-1.501***	-2.508***
	(0.482)	(0.563)
AbsDiff in Household size	0.060	0.153**
	(0.049)	(0.068)
AbsDiff in % hh members (16-59)	0.181	0.011
	(0.272)	(0.354)
AbsDiff in No. of family hh within village	-0.027*	-0.033**
	(0.015)	(0.016)
AbsDiff in No. of family hh outside village	0.003	-0.062
	(0.030)	(0.039)
AbsDiff in Age of head	-0.015**	-0.017**
	(0.007)	(0.007)
AbsDiff in Head completed high school	0.004	0.037
	(0.158)	(0.151)
AbsDiff in Female head	0.068	0.079
	(0.181)	(0.165)
Constant	-2.473***	-0.511
	(0.673)	(0.764)
Ego fixed effects	No	Yes
Alter fixed effects	No	Yes
Observations	4160	3357
log likelihood	-859.128	-758.231
χ^2	225.427	191.690
p	0.000	0.000

Logit Estimation on the directed network. Dyadic robust standard errors in parentheses.

A.2.3 Neediness Score

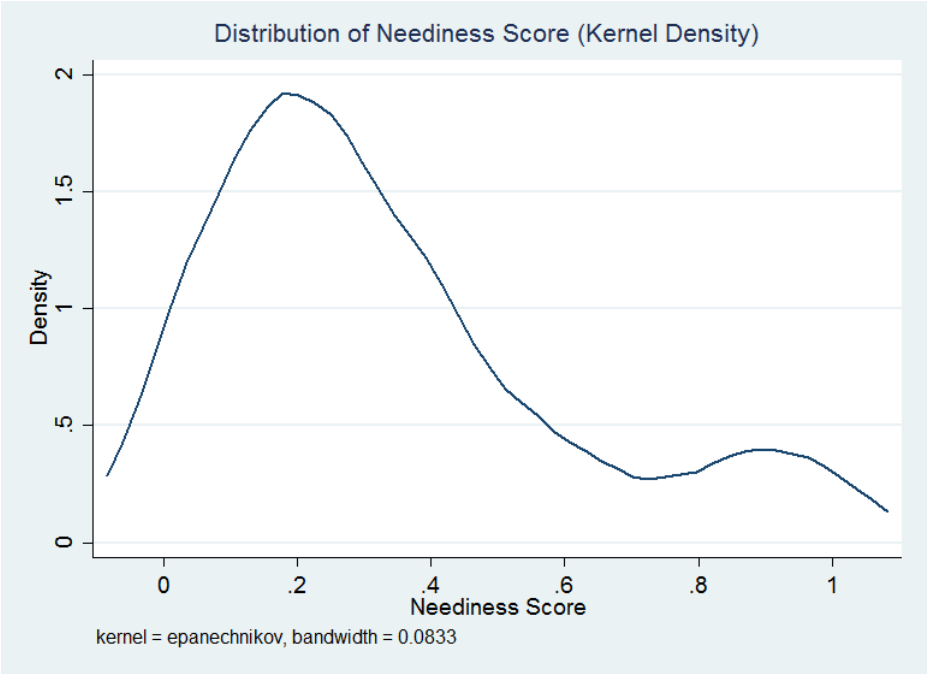


Figure A.1: Distribution of Neediness Score in Maramig

Table A.17: Likelihood of Mutual Support Arrangement - Naïve Approach, full table

	(1) $P(\varsigma_{ij}=1)$	(2) $P(\varsigma_{ij}=1)$
Asset wealth	0.595*	
	(0.330)	
Household size	0.098***	
	(0.038)	
% hh members (16-59)	0.107	
	(0.191)	
No. of family hh within village	0.012	
	(0.009)	
No. of family hh outside village	-0.037	
	(0.023)	
Age of head	-0.011**	
	(0.005)	
Head completed high school	-0.279**	
	(0.118)	
Female head	-0.073	
	(0.145)	
Alter: Asset wealth	0.595*	
	(0.330)	
Alter: Household size	0.098***	
	(0.038)	
Alter: % hh members (16-59)	0.107	
	(0.191)	
Alter: No. of family hh within village	0.012	
	(0.009)	
Alter: No. of family hh outside village	-0.037	
	(0.023)	
Alter: Age of head	-0.011**	
	(0.005)	
Alter: Head completed high school	-0.279**	
	(0.118)	
Alter: Female head	-0.073	
	(0.145)	
AbsDiff in Asset wealth	-0.523	-1.313**
	(0.511)	(0.579)
AbsDiff in Household size	0.108**	0.231***
	(0.047)	(0.077)
AbsDiff in % hh members (16-59)	0.090	-0.431
	(0.309)	(0.406)
AbsDiff in No. of family hh within village	-0.018	-0.020
	(0.017)	(0.018)
AbsDiff in No. of family hh outside village	-0.004	-0.059
	(0.031)	(0.045)
AbsDiff in Age of head	-0.014*	-0.017**
	(0.007)	(0.008)
AbsDiff in Head completed high school	-0.065	-0.093
	(0.178)	(0.167)
AbsDiff in Female head	0.136	0.260
	(0.169)	(0.191)
Constant	-1.860***	-2.492***
	(0.700)	(0.752)
Ego fixed effects	No	Yes
Alter fixed effects	No	Yes
Observations	2080	1854
log likelihood	-647.404	-568.690
χ^2	69.712	151.925
p	0.000	0.030

Logit Estimation on the undirected network. $\varsigma_{ij}=1$ if $s_{ij}=1$ or $s_{ji}=1$.
Dyadic robust standard errors in parentheses.

Table A.18: Likelihood of Mutual Support Arrangement - Accounting for Reciprocation, full table

	(1) $P(\dot{\varsigma}_{ij}=1)$	(2) $P(\dot{\varsigma}_{ij}=2)$	(3) $P(\dot{\varsigma}_{ij}=3)$
Asset wealth	-0.847	1.858***	0.256
	(0.604)	(0.647)	(1.044)
Household size	0.069	0.143***	0.077
	(0.062)	(0.055)	(0.091)
% hh members (16-59)	0.077	-0.010	0.184
	(0.267)	(0.434)	(0.552)
No. of family hh within village	0.025*	-0.011	0.033*
	(0.014)	(0.017)	(0.019)
No. of family hh outside village	-0.020	-0.030	-0.182*
	(0.030)	(0.040)	(0.097)
Age of head	-0.020**	-0.005	0.007
	(0.008)	(0.008)	(0.015)
Head completed high school	-0.483**	-0.120	-0.001
	(0.235)	(0.262)	(0.370)
Female head	-0.188	0.067	0.084
	(0.227)	(0.197)	(0.372)
Alter: Asset wealth	1.858***	-0.847	0.256
	(0.647)	(0.604)	(1.044)
Alter: Household size	0.143***	0.069	0.077
	(0.055)	(0.062)	(0.091)
Alter: % hh members (16-59)	-0.010	0.077	0.184
	(0.434)	(0.267)	(0.552)
Alter: No. of family hh within village	-0.011	0.025*	0.033*
	(0.017)	(0.014)	(0.019)
Alter: No. of family hh outside village	-0.030	-0.020	-0.182*
	(0.040)	(0.030)	(0.097)
Alter: Age of head	-0.005	-0.020**	0.007
	(0.008)	(0.008)	(0.015)
Alter: Head completed high school	-0.120	-0.483**	-0.001
	(0.262)	(0.235)	(0.370)
Alter: Female head	0.067	-0.188	0.084
	(0.197)	(0.227)	(0.372)
AbsDiff in Asset wealth	-0.949*	-0.949*	-4.330***
	(0.525)	(0.525)	(1.200)
AbsDiff in Household size	0.132***	0.132***	-0.136
	(0.045)	(0.045)	(0.158)
AbsDiff in % hh members (16-59)	-0.038	-0.038	0.746
	(0.310)	(0.310)	(0.676)
AbsDiff in No. of family hh within village	-0.014	-0.014	-0.060*
	(0.020)	(0.020)	(0.035)
AbsDiff in No. of family hh outside village	-0.013	-0.013	0.069
	(0.042)	(0.042)	(0.134)
AbsDiff in Age of head	-0.012	-0.012	-0.028*
	(0.008)	(0.008)	(0.015)
AbsDiff in Head completed high school	-0.186	-0.186	0.455
	(0.173)	(0.173)	(0.438)
AbsDiff in Female head	0.188	0.188	-0.197
	(0.184)	(0.184)	(0.363)
Constant	-2.449***	-2.449***	-4.280*
	(0.785)	(0.785)	(2.369)
Observations	2080		
log likelihood	-815.892		
χ^2	308.696		
p	0.000		

Multinomial logit estimation. Dyadic robust standard errors in parentheses.

Table A.19: Likelihood of Mutual Support Arrangement - Neediness Score, full table

	Part (1) Mutual Support			Part (2) Mutual Support		
	$P(\dot{\varsigma}_{ij}=1)$	$P(\dot{\varsigma}_{ij}=2)$	$P(\dot{\varsigma}_{ij}=3)$	$P(\dot{\varsigma}_{ij}=1)$	$P(\dot{\varsigma}_{ij}=2)$	$P(\dot{\varsigma}_{ij}=3)$
$\widehat{needy}$	-0.285	0.287	-0.404	-0.456	0.767	1.506**
	(0.229)	(0.452)	(0.660)	(0.504)	(0.562)	(0.590)
Age of head	-0.027***	0.012	-0.014	-0.029***	0.011	-0.016
	(0.007)	(0.009)	(0.013)	(0.007)	(0.009)	(0.013)
Head compl. high school	-0.646***	0.434	-0.863**	-0.623**	0.479	-0.838**
	(0.250)	(0.310)	(0.381)	(0.257)	(0.324)	(0.398)
Female head	-0.626**	-0.397	0.415	-0.646**	-0.400	0.332
	(0.277)	(0.308)	(0.531)	(0.270)	(0.316)	(0.540)
Alter: $\widehat{needy}$	0.287	-0.285	-0.404	0.767	-0.456	1.506**
	(0.452)	(0.229)	(0.660)	(0.562)	(0.504)	(0.590)
Alter: Age of head	0.004	-0.026***	0.002	0.006	-0.025***	0.007
	(0.009)	(0.008)	(0.015)	(0.008)	(0.008)	(0.015)
Alter: Head compl. high school	0.755**	-0.540**	0.568	0.815***	-0.495*	0.704
	(0.294)	(0.265)	(0.540)	(0.303)	(0.273)	(0.531)
Alter: Female head	0.602***	-0.279	-0.618	0.526***	-0.327	-0.821*
	(0.190)	(0.279)	(0.474)	(0.201)	(0.282)	(0.466)
AbsDiff in $\widehat{needy}$	0.728*	0.728*	-1.366*	0.153	0.153	-1.238
	(0.428)	(0.428)	(0.780)	(0.651)	(0.651)	(1.206)
AbsDiff in Age of head	-0.010	-0.023**	-0.018	-0.011	-0.023**	-0.019
	(0.010)	(0.010)	(0.015)	(0.009)	(0.010)	(0.015)
AbsDiff in Head compl. high school	-0.133	-0.179	0.170	-0.164	-0.223	0.207
	(0.233)	(0.253)	(0.463)	(0.240)	(0.256)	(0.456)
AbsDiff in Female head	0.037	0.421	-0.569	0.087	0.480	-0.559
	(0.177)	(0.290)	(0.480)	(0.185)	(0.292)	(0.469)
commonfriend				1.052**	1.052**	3.990***
				(0.419)	(0.419)	(0.987)
$\widehat{needy}$ * commonfriend				0.273	-0.759	-3.006***
				(0.754)	(0.617)	(1.078)
(Alter: $\widehat{needy}$) * commonfriend				-0.759	0.273	-3.006***
				(0.617)	(0.754)	(1.078)
(Absdiff in $\widehat{needy}$)* commonfriend				0.660	0.660	0.320
				(0.882)	(0.882)	(1.877)
Constant	-1.980***	-1.980***	-2.450*	-2.429***	-2.429***	-5.069***
	(0.655)	(0.655)	(1.419)	(0.719)	(0.719)	(1.530)
Observations	2080			2080		
log likelihood	-839.892			-799.244		
χ^2	166.661			551.357		
p	0.000			0.000		

Multinomial logit estimation. Dyadic robust standard errors in parentheses.

Table A.20: Likelihood of Mutual Support Arrangement - incl. endogenous predictors

	(1) $P(\dot{\varsigma}_{ij}=1)$	(2) $P(\dot{\varsigma}_{ij}=2)$	(3) $P(\dot{\varsigma}_{ij}=3)$
Asset wealth	0.336	4.554**	2.043
	(0.228)	(3.408)	(2.894)
Household size	1.015	1.142**	1.080
	(0.068)	(0.062)	(0.116)
% hh members (16-59)	0.994	1.326	0.975
	(0.302)	(0.590)	(0.528)
No. of family hh within village	1.038**	0.985	1.031
	(0.018)	(0.017)	(0.021)
No. of family hh outside village	0.992	0.981	0.846*
	(0.034)	(0.039)	(0.079)
Health insurance	0.715	1.680**	0.971
	(0.157)	(0.376)	(0.382)
Access to credit	1.618**	0.846	1.029
	(0.338)	(0.250)	(0.369)
Receives remittances	0.732	1.041	0.572*
	(0.153)	(0.249)	(0.186)
Alter: Asset wealth	4.554**	0.336	2.043
	(3.408)	(0.228)	(2.894)
Alter: Household size	1.142**	1.015	1.080
	(0.062)	(0.068)	(0.116)
Alter: % hh members (16-59)	1.326	0.994	0.975
	(0.590)	(0.302)	(0.528)
Alter: No. of family hh within village	0.985	1.038**	1.031
	(0.017)	(0.018)	(0.021)
Alter: No. of family hh outside village	0.981	0.992	0.846*
	(0.039)	(0.034)	(0.079)
Alter: Health insurance	1.680**	0.715	0.971
	(0.376)	(0.157)	(0.382)
Alter: Access to credit	0.846	1.618**	1.029
	(0.250)	(0.338)	(0.369)
Alter: Receives remittances	1.041	0.732	0.572*
	(0.249)	(0.153)	(0.186)
AbsDiff in Asset wealth	0.390*	0.390*	0.011***
	(0.215)	(0.215)	(0.014)
AbsDiff in Household size	1.173***	1.173***	0.893
	(0.058)	(0.058)	(0.143)
AbsDiff in % hh members (16-59)	0.965	0.965	2.088
	(0.307)	(0.307)	(1.449)
AbsDiff in No. of family hh within village	0.986	0.986	0.935*
	(0.020)	(0.020)	(0.035)
AbsDiff in No. of family hh outside village	0.983	0.983	1.054
	(0.044)	(0.044)	(0.138)
AbsDiff in Health insurance	0.841	0.841	1.691
	(0.139)	(0.139)	(0.608)
AbsDiff in Access to credit	0.885	0.885	0.701
	(0.159)	(0.159)	(0.241)
AbsDiff in Receives remittances	0.866	0.866	0.781
	(0.175)	(0.175)	(0.287)
Constant	0.047***	0.047***	0.067
	(0.041)	(0.041)	(0.172)
Observations	2080		
Control variables	Yes		
log likelihood	-800.971		
χ^2	3641.291		
p	0.000		

Multinomial logit estimation. Dyadic robust standard errors in parentheses.
Control for the level of and absolute differences in Age of head, Education of head, Female head.

Table A.21: Predicting Neediness

	(1)
Female members between 0 and 5	1.503*
	(0.322)
Female members between 6 and 15	0.455***
	(0.125)
Female members between 16 and 49	0.556***
	(0.097)
Female members between 50 and 69	0.447*
	(0.185)
Female members above 69	0.864
	(0.420)
Male members between 0 and 5	2.579***
	(0.753)
Male members between 6 and 15	0.956
	(0.182)
Male members between 16 and 49	1.812**
	(0.445)
Male members between 50 and 69	1.606
	(0.570)
Male members above 69	3.503**
	(1.809)
Household head no basic education	0.957
	(0.270)
Female head	1.074
	(0.356)
Age of head	0.979*
	(0.011)
No. of family hh outside village	0.974
	(0.052)
Village fixed effects	Yes
Observations	303
Mean of Dependent Variable	0.380
log likelihood	-162.605
r^2_p	0.192

Logit estimation; estimators reported as odds ratios; standard errors clustered on village level in parantheses. Based on the sample of 306 surveyed households in the 22 villages, including a random sample of 14 households from Maramig.

Table A.22: Neediness Score for Maramig

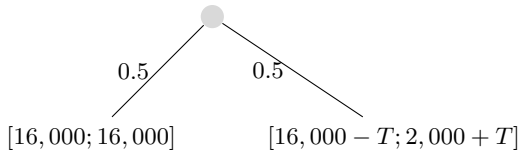
	mean	sd	min	max	median
$\widehat{needy}$	0.34	0.26	0	1	.27
Observations	65				

Appendix B

Appendix for Chapter 3

B.1 Game Design

Outcome without insurance option [provider;recipient]



Outcome with insurance option [provider;recipient]

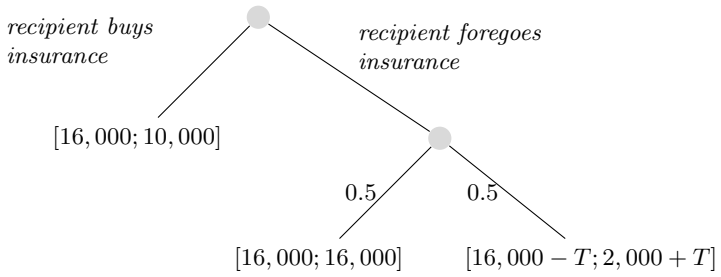


Figure B.1: Outcome Tree of Transfer Game

B.2 Instructions

B.2.1 Instruction for the 'General Introduction'

[All 32 participants sit, at the front two RAs, the other RAs stand ready with their color sign boards]

Thank you all for coming today. My name is XXX. Let me briefly introduce our team to you. [*Introduce each RA.*] And this is Friederike who is a researcher at a university in Germany.

This workshop today has 2 games and 6 rounds in total. During the workshop you can earn a considerable amount of money that you are permitted to keep and take home. In the six different rounds you will have to make decisions that will influence your personal earning, but each of you will be given a show-up fee of 4,000 Riel at the end for sure. [*Show money.*] The remaining procedure, from now on, will last around three hours. Thank you in advance for your effort and time.

You should understand that the money you can earn in this workshop is not Friederike's own money. It is money given to her by the German government to do a research study. Friederike is working together with other researchers who are carrying out similar workshops all around the world.

If at any time you find that this is something that you do not wish to participate in for any reason, you are of course free to leave whether we have started the game or not. If you already feel uncomfortable, or you already know that you will not be able to stay for the three hours, then you should tell us now.

It is very important that you understand each round. Therefore we will check your understanding by asking each of you test questions about the rules. If you do not understand the rules you may ask the assistants to explain them. But if you cannot answer the test questions after explaining them twice, we will have to exclude you from the workshop and you receive only the show-up fee. But don't worry: we will do our best to help you understand.

The workshop is structured as follows: we have one game with two rounds, then a break during which you will be asked to answer a short questionnaire and then a second game with four rounds. After this there will be the payout. It is very important for our research that you answer all questions of the questionnaire seriously.

After knowing these rules, is there anybody who does not like to participate? [*Wait some moments.*]

There will be six rounds that are slightly different from each other. At the beginning of each round, each of you will be given 16,000 Riel as endowment. These 16,000 Riel are play money. But they will be exchanged to real money at the end of the workshop. In each round you might lose some of this money. How much you keep and eventually your final earnings of this workshop depend on your decisions, decisions of others and luck. The show-up fee of 4,000 Riel is always untouched. We will at no time inform you about the outcome of other participants.

Friederike administers the accounts for each participant. [*Show template sheet with accounts for each participant.*] After each round, the amount each participant earned will be reported to Friederike. We play 6 rounds. Each round is named after a fruit. We have Mango, Pineapple,

Orange 1, Orange 2, Apple 1, Apple 2. But you might play them in a different order. Only one of the 6 rounds determines the final payout for you. At the end of the workshop, we will draw a ball to determine which of the 6 rounds will be paid out to you. [*Show 6 balls with the fruit names.*] Just one of the 6 rounds is finally paid out. [*Shows example sheet with accounts for each participants.*] This is an example for the account sheet. Each row specifies the outcome for a specific participant in each round. [*Show.*] Let us assume we draw this ball. [*Draw a ball, show the name on it.*] How much will be the payout of this participant? [*Show participant number on sheet.*] And how much of this? [*Show a different number.*] Let us assume we draw this ball. [*Draw a ball, show the name on it.*] How much will be the payout of this participant? [*Show participant number on sheet.*] And how much of this? [*Show a different number.*] [*Repeat until understood.*]

The outcomes in one round have absolutely no influence on the outcomes of another round or another game. They are completely independent from each other. So, if you make your decision in one round, don't worry what happened in the rounds before or what will happen in the following rounds. Just take each round seriously on its own, because it might be the one that is paid out.

In the rounds you have to make decisions about small sums of money. Each decision you make is good – there are no wrong decisions. Your decisions will be kept in private, so just choose the option YOU like best!

After we finished the six rounds, one by one will come to Friederike, who will hand out the earnings of the round drawn plus the show-up fee to you and you sign the receipt.

You all received a colour badge and a participant number. The participant number is your personal number. You keep this number for all six rounds of the workshop and have to show it at the end in order to get paid. So always remember to take the color badge with you.

There are some more rules for communication. During the rounds talking is strictly prohibited. You cannot ask questions to the other participants or talk about the rules with other participants while we are in the process of the round. If you have any questions, please raise your hand and wait until someone comes to answer your question in private. If you do not follow the rule you cannot participate in the workshop anymore and get no earnings from the workshop.

We will now start. Please go to the assistant that shows a signboard with your colour. This is your group. For all 6 rounds of this workshop you will stay in this group.

[*RAs collect the participants, go with them to the respective rooms.*]

B.2.2 Instruction - Providers A1

Welcome again. I am YYY and this is ZZZ. We will assist you in this round. Let me reiterate what XXX stated in the introduction: Talking is strictly prohibited. You must not ask questions to the other participants or talk about the rules with other participants while the round is in progress. If you have any questions, please raise your hand and wait until I or my colleague comes to answer your question in private. If you do not follow the rule you cannot participate in the workshop anymore and get no earnings from the workshop.

Let me first hand out your endowment. [*Hand out bags with money to each participant.*] You will find in the bag 16,000 Riel in play money. This is your endowment. Remember, while we use play money now, this translates to real money later.

You are lucky; you can keep all your money. This was determined through a random draw that allocated participants to their roles. People in the other rooms are less lucky. They also receive 16,000 Riel in play money. But then each of them rolls the dice – like this [*Show.*]. If the dice shows 4, 5 or 6, they can keep their 16,000 Riel. But if the dice shows 1, 2 or 3 they lose 14,000 Riel and can keep only 2,000 Riel.

Each of you will be matched randomly with one partner from the other rooms. You can support your partner in the case that she loses 14,000 Riel. Thus, you can decide whether you want to transfer part of your money to your partner if she rolls a 1, 2 or 3, and therefore suffers a loss of 14,000 Riel. Please note that you will *never* be informed about the name of your partner.

Your partner *does not know* that she has a partner. She *does not know* that she might receive support from someone in case of a loss. If you decide to transfer something in case you partner loses, this amount will be added to her account. But she will *never* be informed about you and your name.

There are two different groups [*Show on illustration.*] and your partner is in one of the two groups:[1]

In the group *'khâ'* the participants have the possibility to purchase a guarantee before they play the dice game. The guarantee costs 6,000 Riel. They receive a guarantee certificate. [*Show example.*] If they purchase the guarantee, they have only 10,000 Riel left, but they will not lose anything in the dice game. That is, nothing happens to them if the dice falls on 1, 2, or 3.

In the other group, *'kâ'*, participants do not have the possibility to buy a guarantee. They just play the dice game and they will either lose money or not.

[*Round 1: Test Questions Simulation Group 'khâ' and Test Questions Simulation Group 'kâ'*]

Each of you will be matched randomly with a partner in either the 'khâ' group or in the 'kâ' group. Before the partner matching is determined, you are therefore asked to make two independent decisions: "Suppose you have your partner in group *'kâ'* and your partner loses 14,000 Riel in the dice game, thus she has only 2,000 Riel left. Will you transfer part of your money to your partner? If so, how much?" You will note this down in private on a sheet. [*Show the sheet.*]

And then: "Suppose on the other side, your partner is not in group 'kâ', but in group *'khâ'*. Thus your partner had the option to purchase a guarantee. However, your partner *decided not* to purchase the guarantee and loses 14,000 Riel in the dice game, thus she has only 2,000 Riel left. Will you transfer part of your money to your partner? If so, how much?" You will note this down in private on another sheet [*Show sheet*]. You will then still have time to look through both decisions.

For each of you we then determine in which group your partner is. You will receive an envelope. You put the amount of money that you decided to transfer to your partner into the

[1] We used two Khmer letters to refer to the recipients without the insurance option and to those with the insurance option, letter 'kâ' for recipients of type $B1$ and $C1$ and letter 'khâ' for recipients of type $B2$ and $C2$.

envelope. The amount of money must be the exact same as noted on the decision sheet. The amount will be double checked. In case there is any difference the amount you indicated on the sheet will determine the transfer. After you put the amount in the envelope, we will collect the envelopes. The money in the envelope will be transferred to your partner in case she loses part of her endowment. If she does not lose anything, you will keep the money in the envelope. All remaining money will determine your personal earnings for this game.

[*Round 1 and Round 2: Test Questions Transfers*]

Your decisions are anonymous in two ways: First, your name will never be revealed to your partner and your partner's name will never be revealed to you. Second, you will do your transfer decision in private using this cardboard [*Show cardboard.*]. We will not look at the decision sheets or on the envelopes. In fact, we will not even touch the sheets or envelopes. You will put them in this basket [*Show basket.*] and we will bring the basket directly to Friederike. Hence your transfer decisions will not be observed by the other participants and not by us.

Remember, that the transfer decision is yours and only yours – there are no wrong decisions. You can transfer nothing or 1,000 Riel, 2,000 Riel, 3,000 Riel, etc. as you wish. Your transfers will be kept in private, *so just choose the amount YOU like best! And remember it's real money!*

Remember, you partner does not know that she has a partner. She is not expecting anything.

Let us start. [*Hand out the decision sheet for each participant and pencils.*] Please assume your partner is in Group *'kâ'*, thus your partner has no possibility to buy a guarantee. She rolls the dice and loses 14,000 Riel. She has only 2,000 Riel left. Would you like to transfer part of your 16,000 Riel? Please write in the box how much money you would like to transfer. If you do not want to transfer anything you write 0. [*Wait.*] Please put the sheet next to you.

[*Hand out the other decision sheet to each participant.*] Now, please assume your partner is in Group *'khâ'*, your partner decided *not* to purchase the guarantee, she rolls the dice and loses 14,000 Riel. She only keeps 2,000 Riel. Would you like to transfer part of your 16,000 Riel? Please write in the box how much money you would like to transfer. If you do not want to transfer anything you write 0. [*Wait.*]

Now, please have a look at your two decisions. One of the two decisions will be enacted in case your partner loses 14,000 Riel. Are you satisfied with your decisions? Then please fold the sheets and lay them in front of you like this [*Show with name on the top.*]. [*Collect pencils.*]

I will now hand out the partner envelope. [*Go from one participant to the other, each draws one white envelope with the partner's group name and a player id written on it.*] Please look at the envelope. It tells you in which group your partner is. Please now take your decision sheet for this group [*Collect the other decision sheet which is not relevant with a basket.*], look at it and add the money exactly according to your decision in the envelope. This amount will be transferred to your partner if she loses. [*Wait. Then ask each participant to put the envelope in a basket and hand out brown envelopes with the player number of the player.*] Now, here you put your remaining money. This will be transferred to your account.

[*Ask each participant to put the envelope in the other basket. Finish.*]

B.2.3 Instruction - Providers A2

Welcome again. I am YYY and this is ZZZ. We will assist you in this round. Let me reiterate what XXX stated in the introduction: Talking is strictly prohibited. You must not ask questions to the other participants or talk about the rules with other participants while the round is in progress. If you have any questions, please raise your hand and wait until I or my colleague comes to answer your question in private. If you do not follow the rule you cannot participate in the workshop anymore and get no earnings from the workshop.

Let me first hand out your endowment. [*Hand out bags with money to each participant.*] You will find in the bag 16,000 Riel in play money. This is your endowment. Remember, while we use play money now, this translates to real money later.

You are lucky; you can keep all your money. This was determined through a random draw that allocated participants to their roles. People in the other rooms are less lucky. They also receive 16,000 Riel in play money. But then each of them rolls the dice – like this [*Show.*]. If the dice shows 4, 5 or 6, they can keep their 16,000 Riel. But if the dice shows 1, 2 or 3 they lose 14,000 Riel and can keep only 2,000 Riel.

Each of you will be matched randomly with one partner from the other rooms. You can support your partner in the case that she loses 14,000 Riel. Thus, you can decide whether you want to transfer part of your money to your partner if she rolls a 1, 2 or 3, and therefore suffers a loss of 14,000 Riel. Please note that you will *never* be informed about the name of your partner.

Your partner knows that she might receive support from someone in this group in case of a loss. They are asked how much they expect to receive. But they, too, will *never* be informed about your name.

There are two different groups [*Show on illustration.*] and your partner is in one of the two groups:

In the group *'khâ'* the participants have the possibility to purchase a guarantee before they play the dice game. The guarantee costs 6,000 Riel. They receive a guarantee certificate. [*Show example.*] If they purchase the guarantee, they have only 10,000 Riel left, but they will not lose anything in the dice game. That is, nothing happens to them if the dice falls on 1, 2, or 3.

In the other group, *'kâ'*, participants do not have the possibility to buy a guarantee. They just play the dice game and they will either lose money or not.

[*Round 1: Test Questions Simulation Group 'khâ' and Test Questions Simulation Group 'kâ'*]

Each of you will be matched randomly with a partner in either the 'khâ' group or in the 'kâ' group. Before the partner matching is determined, you are therefore asked to make two independent decisions: "Suppose you have your partner in group *'kâ'* and your partner loses 14,000 Riel in the dice game, thus she has only 2.000 Riel left. Will you transfer part of your money to your partner? If so, how much?" You will note this down in private on a sheet. [*Show the sheet.*]

And then: "Suppose on the other side, your partner is not in group 'kâ', but in group *'khâ'*. Thus your partner had the option to purchase a guarantee. However, your partner decided *not* to purchase the guarantee and loses 14,000 Riel in the dice game, thus she has only 2,000 Riel

left. Will you transfer part of your money to your partner? If so, how much?" You will note this down in private on another sheet [*Show sheet*]. You will then still have time to look through both decisions.

For each of you we then determine in which group your partner is. You will receive an envelope. You put the amount of money that you decided to transfer to your partner into the envelope. The amount of money must be the exact same as noted on the decision sheet. The amount will be double checked. In case there is any difference the amount you indicated on the sheet will determine the transfer. After you put the amount in the envelope, we will collect the envelopes. The money in the envelope will be transferred to your partner in case she loses part of her endowment. If she does not lose anything, you will keep the money in the envelope. All remaining money will determine your personal earnings for this game.

[*Round 1 and Round 2: Test Questions Transfers*]

Your decisions are anonymous in two ways: First, your name will never be revealed to your partner and your partner's name will never be revealed to you. Second, you will do your transfer decision in private using this cardboard [*Show cardboard.*]. We will not look at the decision sheets or on the envelopes. In fact, we will not even touch the sheets or envelopes. You will put them in this basket [*Show basket.*] and we will bring the basket directly to Friederike. Hence your transfer decisions will not be observed by the other participants and not by us.

Remember, that the transfer decision is yours and only yours – there are no wrong decisions. You can transfer nothing or 1,000 Riel, 2,000 Riel, 3,000 Riel, etc. as you wish. Your transfers will be kept in private, *so just choose the amount YOU like best! And remember it's real money!*

Let us start. [*Hand out the decision sheet for each participant and pencils.*] Please assume your partner is in Group *'kâ'*, thus your partner has no possibility to buy a guarantee. She rolls the dice and loses 14,000 Riel. She has only 2,000 Riel left. Would you like to transfer part of your 16,000 Riel? Please write in the box how much money you would like to transfer. If you do not want to transfer anything, you write 0. [*Wait.*] Please put the sheet next to you.

[*Hand out the other decision sheet to each participant.*] Now, please assume your partner is in Group *'khâ'*, your partner decided *not* to purchase the guarantee, she rolls the dice and loses 14,000 Riel. She only keeps 2,000 Riel. Would you like to transfer part of your 16,000 Riel? Please write in the box how much money you would like to transfer. If you do not want to transfer anything you write 0. [*Wait.*]

Now, please have a look at your two decisions. One of the two decisions will be enacted in case your partner loses 14,000 Riel. Are you satisfied with your decisions? Then please fold the sheets and lay them in front of you like this [*Show with name on the top.*]. [*Collect pencils.*]

I will now hand out the partner envelope. [*Go from one participant to the other, each draws one white envelope with the partner's group name and a player id written on it.*] Please look at the envelope. It tells you in which group your partner is. Please now take your decision sheet for this group [*Collect the other decision sheet which is not relevant with a basket.*], look at it and add the money exactly according to your decision in the envelope. This amount will be transferred to your partner if she loses. [*Wait. Then ask each participant to put the envelope in a basket and hand out brown envelopes with the player number of the A player.*] Now, here you

put your remaining money. This will be transferred to your account.

[*Ask each participant to put the envelope in the other basket. Finish.*]

B.2.4 Illustrations

Figure B.2: Illustration for Provider A1 - Recipient B1

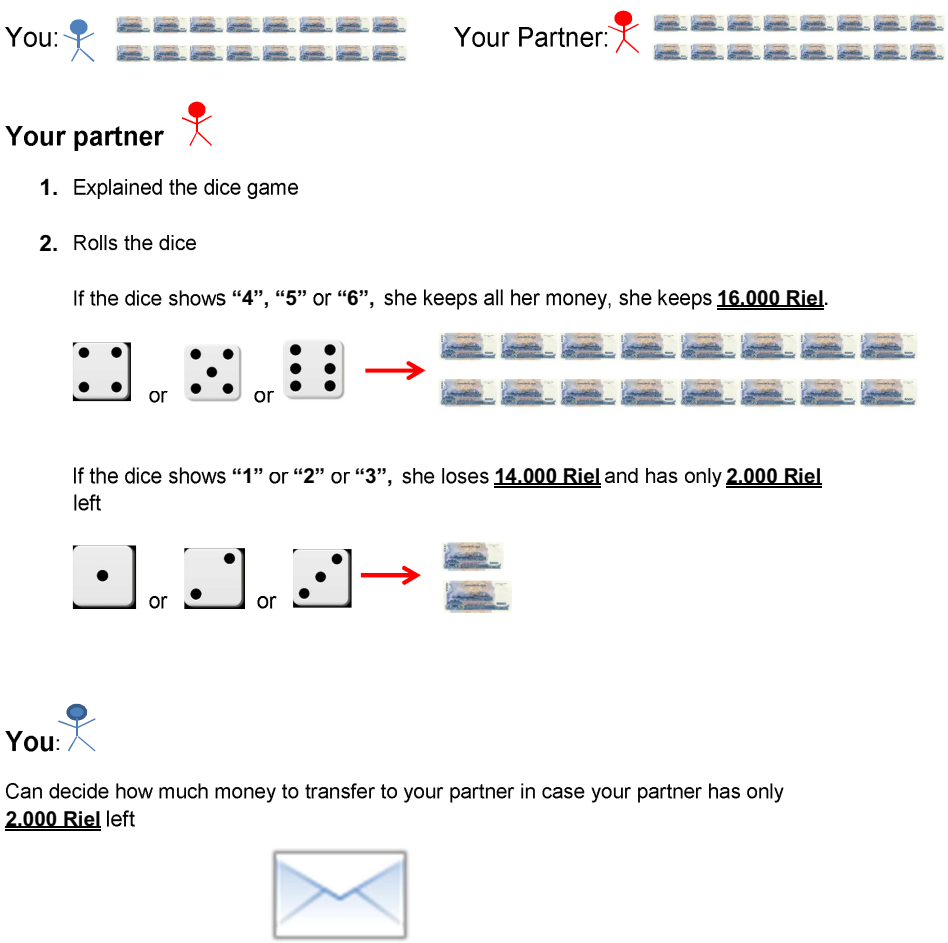


Figure B.3: Illustration for Provider A1 - Recipient B2

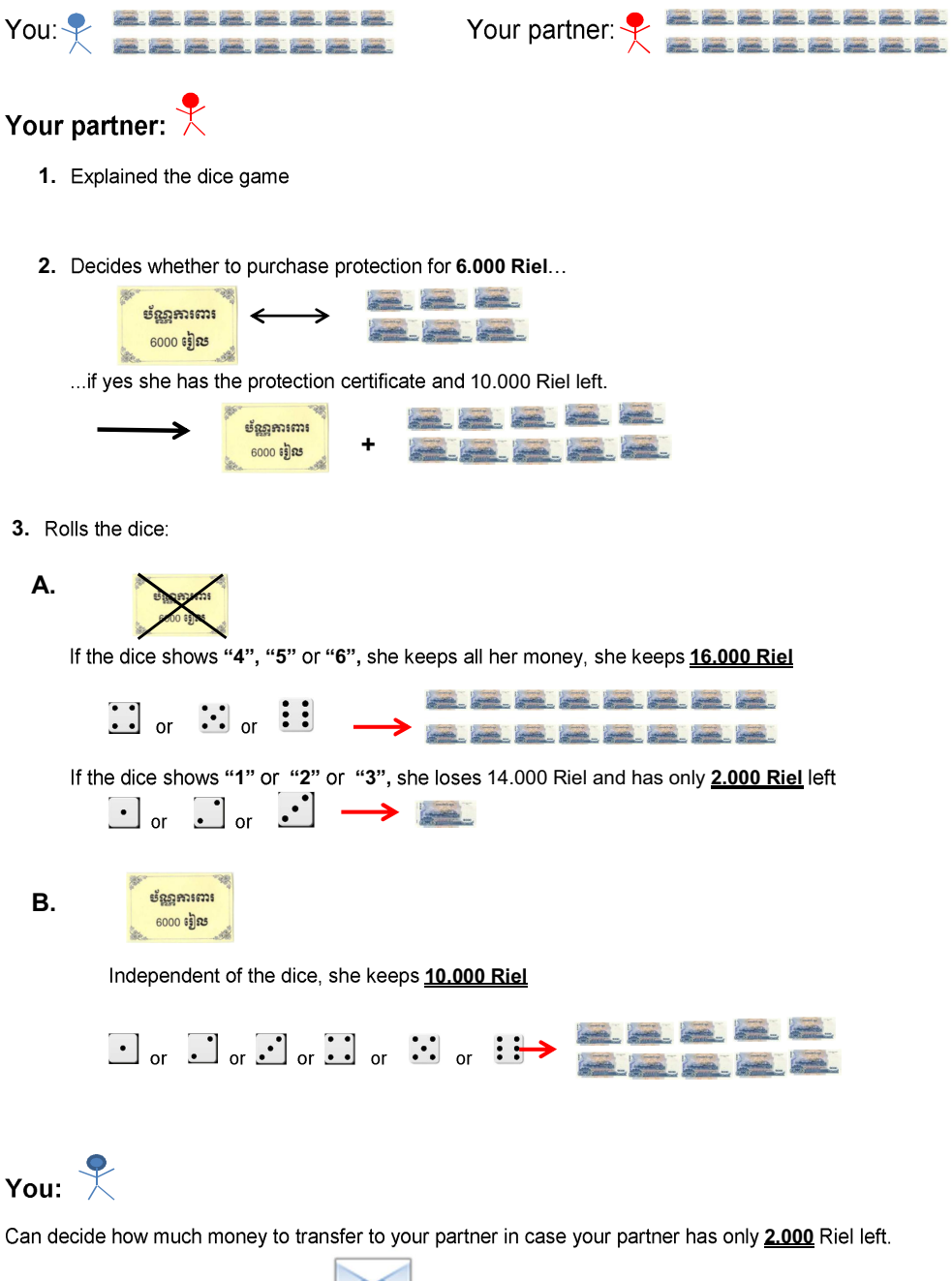


Figure B.4: Illustration for Provider A2 - Recipient C1

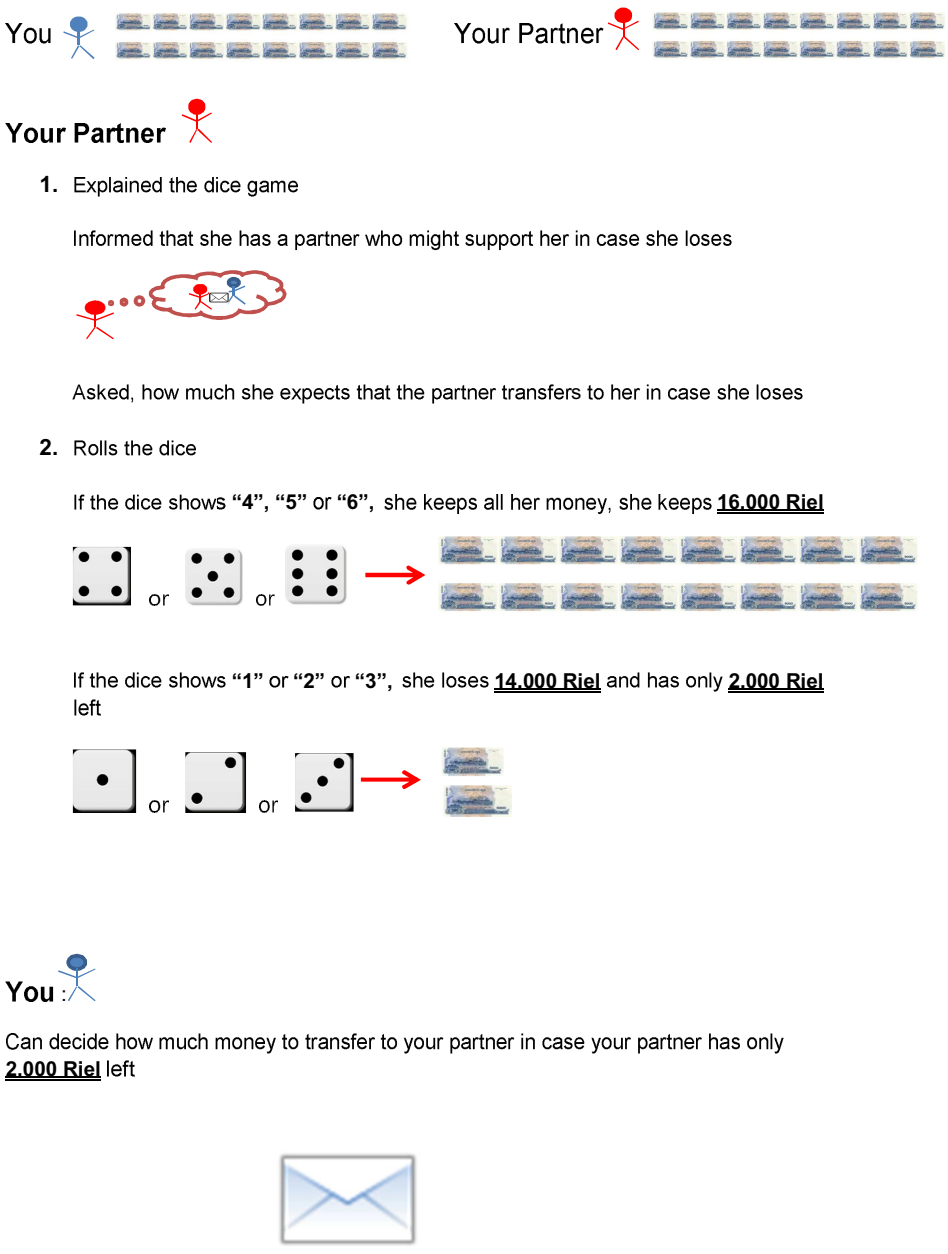
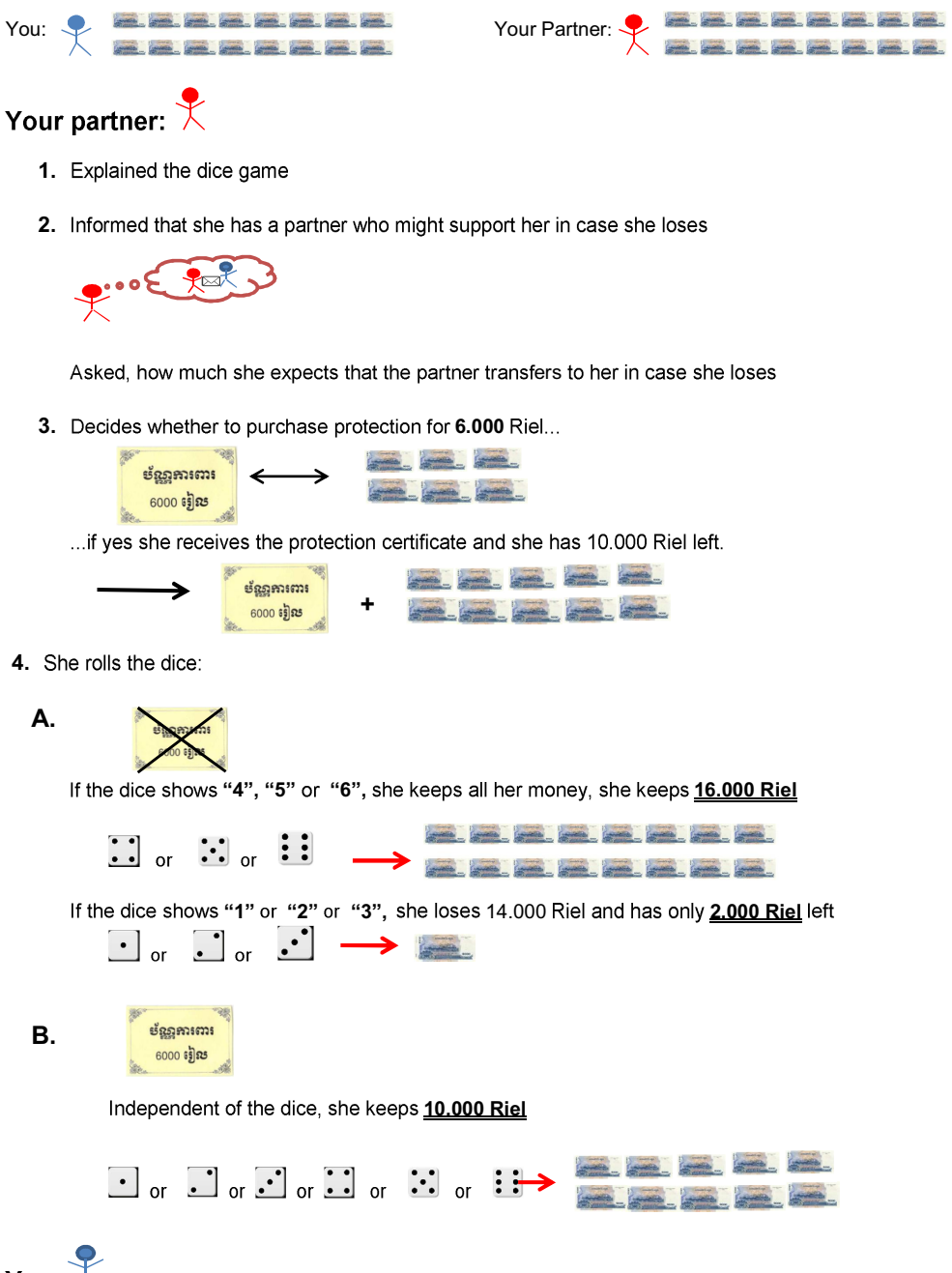


Figure B.5: Illustration for Provider A2 - Recipient C2



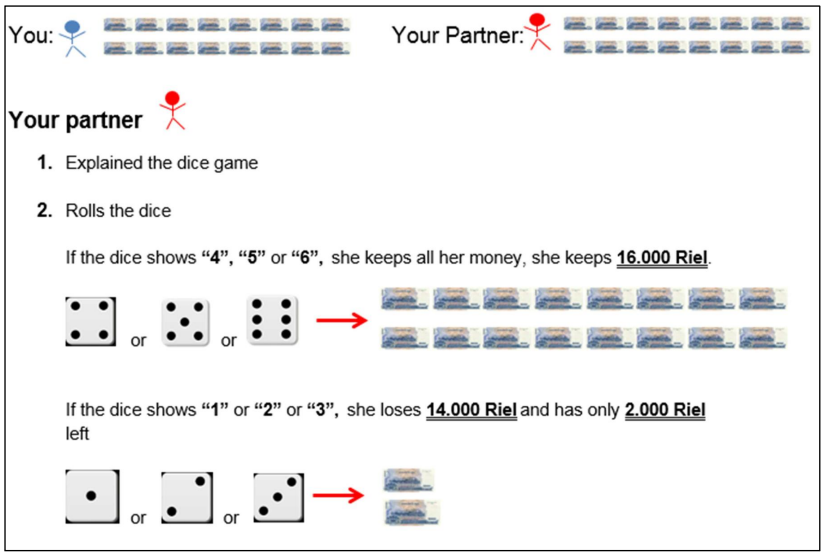
B.2.5 Decision Sheets

Figure B.6: Decision Sheet for Provider A1 - Recipient B1

Game 1 - DS_NG_C - Decision Sheet - ⛩ - Room 6 - Group 1c and 1d - Rose and Orange - RA 7, RA 8

Participants ID:___

Your Partner is in **Group** ⛩



Decision:

Suppose your partner rolls the dice and loses 14.000 Riel. She only keeps 2.000 Riel. Would you like to transfer part of your money?

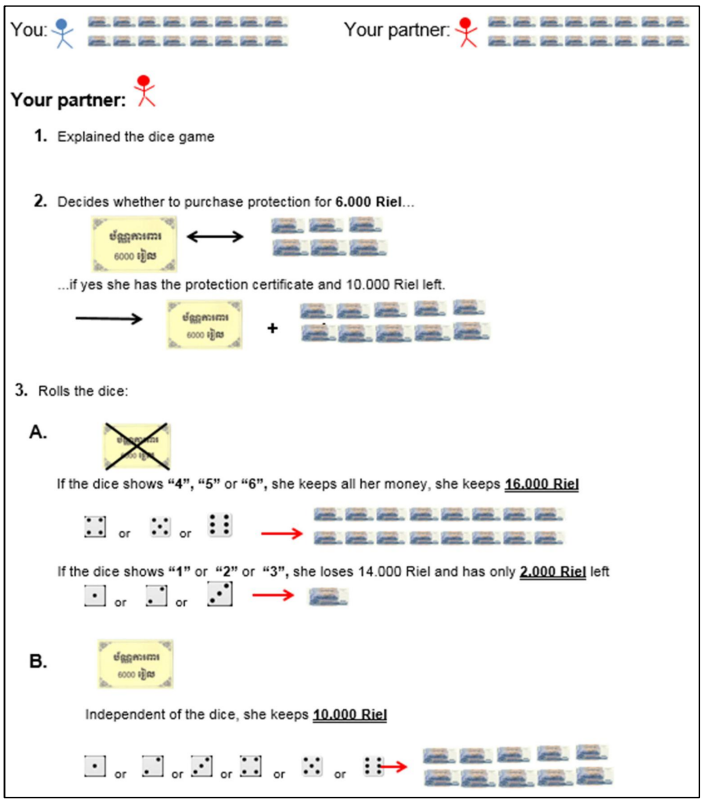
Please write in the box how much you would like to transfer (write in terms of 1.000 Riel). If you do not wish to transfer anything, you write "0". Remember, this is real money!

Figure B.7: Decision Sheet for Provider A1 - Recipient B2

Game 1 - DS_G_C - Decision Sheet - 2 - Room 6 - Group 1c and 1d - Rose and Orange - RA 7, RA 8

Participants ID:___

Your Partner is in **Group 2**



Decision:

Suppose your partner did **not** purchase protection. She rolls the dice and loses 14.000 Riel. She only keeps 2.000 Riel. Would you like to transfer part of your money?

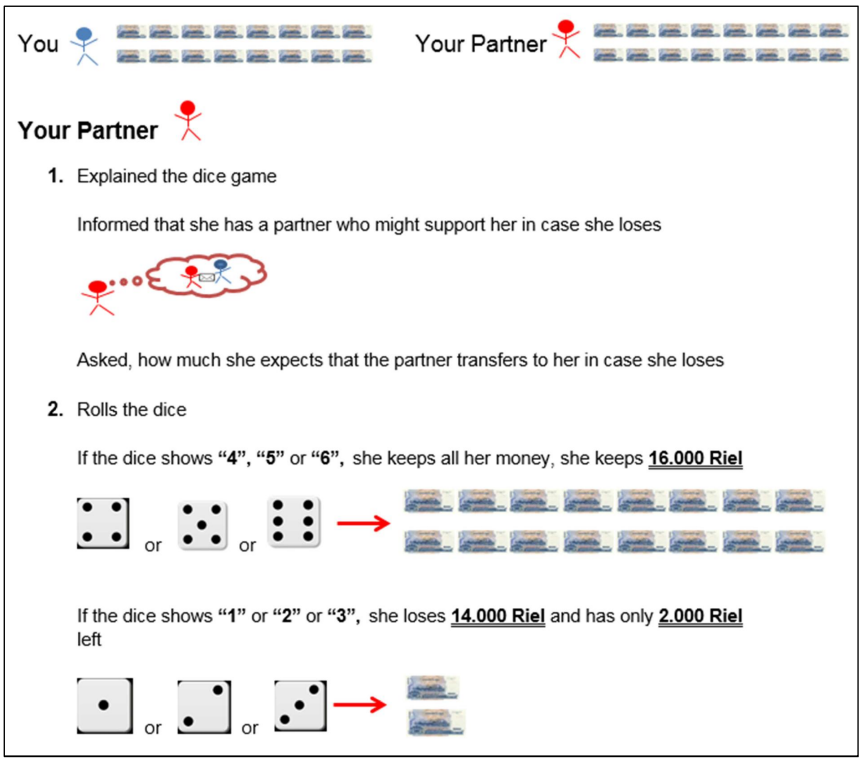
Please write in the box how much you would like to transfer (write in terms of 1.000 Riel). If you do not wish to transfer anything, you write "0". Remember, this is real money!

Figure B.8: Decision Sheet for Provider A2 - Recipient C1

Game 1 - DS_NG - Decision Sheet - 🏠 - Room 5 - Group 1a and 1b - Blue and Purple - RA 5, RA 6

Participants ID:___

Your Partner is in **Group** 🏠



Decision:

Suppose you partner rolls the dice and loses 14.000 Riel. She only keeps 2.000 Riel. Would you like to transfer part of your money?

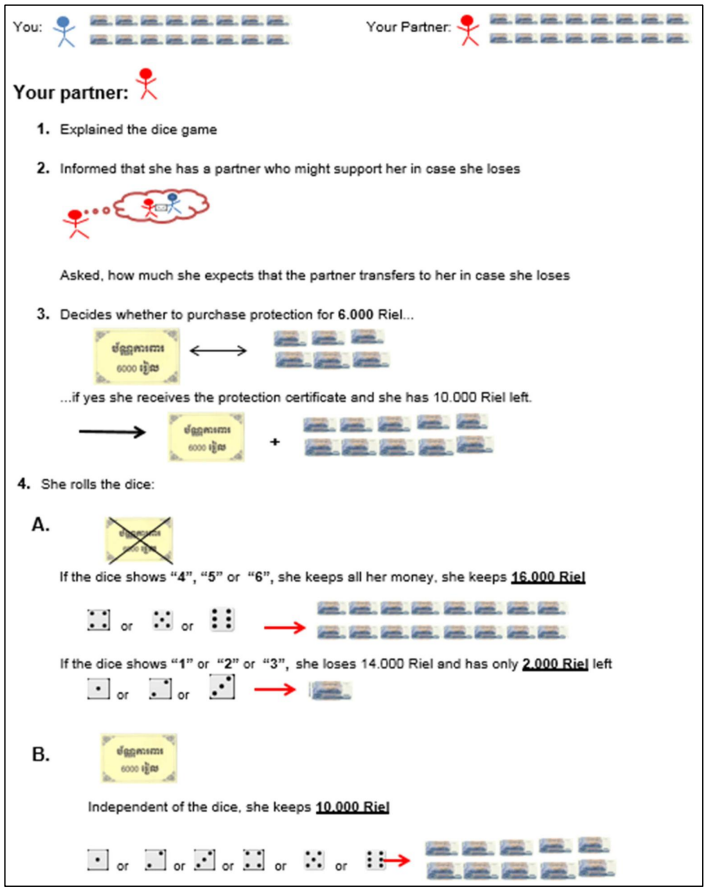
Please write in the box how much you would like to transfer (write in terms of 1.000 Riel). If you do not wish to transfer anything, you write "0". Remember, this is real money!

Figure B.9: Decision Sheet for Provider A2 - Recipient C2

Game 1 - DS_G - Decision Sheet - A - Room 5 - Group 1a and 1b - Blue and Purple - RA 5, RA 6

Participants ID:___

Your Partner is in **Group B**



Decision:

Suppose you partner did **not** purchase protection. She rolls the dice and loses 14.000 Riel. She only keeps 2.000 Riel. Would you like to transfer part of your money?

Please write in the box how much you would like to transfer (write in terms of 1.000 Riel). If you do not wish to transfer anything, you write "0". Remember, this is real money!

B.3 Descriptive Statistics

B.3.1 Characteristics of Experimental Participants

Table B.1 provides an overview of the socio-economic characteristics of the experimental participants. The information is based on the data collected in the survey that was conducted two weeks before the experiment. The participants are a homogenous group in terms of ethnicity and religion, with all but a few being Khmer and Buddhists (not displayed in the table). 68% of the participants are female. In all but two villages, the female participants outweigh the male participants. The imbalance is largely due to the fact that men are more likely to work outside the village (either abroad or within Cambodia); furthermore, at the time of the survey many rice farmers were engaged in rice transplantation which is typically done by men. Participants are between 18 and 77 years old with a mean age of 39 and a median age of 37. Most participants (86%) are married and about half (45%) head their respective households. Two thirds were born in the village where they are now living. The level of education is rather low. The majority of the participants went less than three years to school; 30% never attended school. Correspondigly, only 66% of the participants report to be able to read and write in Khmer. Most of the participants (86%) are self-employed, the majority as rice farmers.

Table B.1: Characteristics of the Experiment Participants

	mean	sd	min	max	median
Individual Characteristics					
Female	0.68	0.47	0	1	1
Age	39.46	12.15	18	77	37
Married	0.86	0.34	0	1	1
Household Head	0.45	0.50	0	1	0
Born in this village	0.67	0.47	0	1	1
Literate	0.66	0.47	0	1	1
Schooling years	2.91	3.02	0	16	2
Self-employed	0.86	0.35	0	1	1
Household Characteristics					
Household size	5.65	2.34	1	18	5
Monthly income (USD)	160.66	363.05	0	5,000	50
ID Poor status	0.21	0.41	0	1	0
Indebted	0.60	0.49	0	1	1
Borrowed from other households	0.44	0.50	0	1	0
Borrowed from financial institution	0.33	0.47	0	1	0
Bank account	0.05	0.21	0	1	0
Member in a saving group	0.20	0.40	0	1	0
Insurance	0.08	0.28	0	1	0
Migrant	0.57	0.49	0	1	1
Remittances	0.51	0.50	0	1	1
Landownership (ha)	2.67	2.26	.016	30	2
No electricity	0.71	0.45	0	1	1
Observations	672				

A household consists on average of six people. Many of the participants are poor. One in two participants report a household income of 50 USD or less in the last month. 21% of the participants come from a household that is officially classified as poor.[2] There is a substantial amount of formal and informal borrowing. 60% of the participants' households have outstanding loans. 44% report to have borrowed money from another household in the village in the last 2 years; 33% borrowed from a financial institution, typically a microfinance institution. Only a small proportion (5%) have a bank account but one fifth participate in an informal savings group. 9% have a formal insurance (mostly, health insurance). The majority of participants (57%) live in a household where at least one household member either worked abroad in the past 2 years or is currently working abroad. 51% of the households receive remittances. The households of all participants own land, with greatly varying land sizes. Average land size is 2.7 hectares. More than two thirds of the participants live in a household without access to electricity.

[2]The so-called IDPoor program was established in Cambodia in 2006 and was meant to provide information on the poor population to facilitate targeting of state programs and NGO assistance. The poverty status is determined based on observable assets, familiy composition and exposure to shocks and is renewed every 3-4 years. Being identified as poor provides, in particular, free access to basic health services.

Table B.2: Mean Comparison Test: Differences in Characteristics for A1 and A2 Providers

Individual Characteristics		
Female	0.02	(0.64)
Age	2.07	(0.12)
Married	-0.02	(0.64)
Household Head	0.02	(0.66)
Born in this village	0.01	(0.91)
Literate	-0.02	(0.64)
Schooling years	-0.15	(0.64)
Self-employed	-0.04	(0.33)
Household Characteristics		
Household size	-0.37	(0.17)
Monthly income (USD)	-24.73	(0.55)
ID Poor status	-0.05	(0.27)
Indebted	-0.04	(0.44)
Borrowed from other households	-0.05	(0.33)
Borrowed from financial institution	-0.01	(0.91)
Bank account	0.05**	(0.03)
Member in a saving group	0.02	(0.58)
Insurance	-0.03	(0.35)
Migrant	0.04	(0.51)
Remittances	0.07	(0.31)
Landownership (ha)	-0.08	(0.77)
No electricity	-0.01	(0.81)
Observations	336	

p-values in parentheses

Provider types are based on the roles subjects played in Round 1

B.4 Treatment Effect Analysis

B.4.1 Tobit Estimation

Table B.3: Treatment Effect Analysis - Tobit Random Effect

	(1) Transfer	(2) Transfer	(3) Transfer	(4) Transfer	(5) Transfer	(6) Transfer
Inf (β)	0.022	0.023	0.035	0.037	0.118	0.114
	(0.090)	(0.089)	(0.099)	(0.106)	(0.103)	(0.072)
Opt (γ)	-0.751***	-0.751***	-0.747***	-0.830***	-0.878***	-0.653***
	(0.092)	(0.091)	(0.101)	(0.108)	(0.106)	(0.074)
InfOpt (η)	0.024	0.026	-0.060	-0.006	-0.005	-0.016
	(0.130)	(0.128)	(0.142)	(0.152)	(0.149)	(0.104)
Constant (θ)	2.102***	2.237***	2.272***	2.324***	2.239***	2.086***
	(0.093)	(0.098)	(0.109)	(0.118)	(0.128)	(0.084)
sigma_u						
Constant (θ)	1.247***	1.250***	1.298***	1.277***	1.208***	1.125***
	(0.060)	(0.060)	(0.067)	(0.074)	(0.082)	(0.053)
sigma_e						
Constant (θ)	1.160***	1.149***	1.184***	1.117***	0.883***	0.922***
	(0.028)	(0.028)	(0.031)	(0.033)	(0.033)	(0.023)
Round effects	No	Yes	Yes	Yes	Yes	Yes
Observations	1344	1344	1168	908	596	1320
log likelihood	-2253.435	-2244.583	-1977.033	-1499.709	-882.897	-1992.970
ρ	0.536	0.542	0.546	0.567	0.652	0.599

Tobit Random effect estimator; bounded at 0. Standard errors in parentheses.

(1)-(2) for all subjects; (3) excluding subjects who needed support in writing;

(4) excluding subjects who made at least two mistake at test questions;

(5) excluding subjects who made at least one mistake at test questions;

(6) excluding subjects who made at least one transfer above 7,000 Riel.

Transfers in terms of 1,000 Riel

B.4.2 Round Effects

In Section 3.4.1, we noticed that there are round effects in our experiment but these round effects did not seem to influence our coefficients of interest. To provide further evidence, we here split our sample by rounds and run the OLS estimation of specification (3.2) separately for Round 1 (Column 1) and Round 2 (Column 2). Table B.4 reports the results.

Table B.4: Treatment Effect Analysis, by Round

	(1) Transfer - Round 1	(2) Transfer - Round2	(1) - (2) Effect Comparison
Inf (β)	-0.268	0.274	-0.542
	(0.277)	(0.282)	(0.395)
Opt (γ)	-0.565***	-0.631***	0.066
	(0.123)	(0.166)	(0.206)
InfOpt (η)	-0.048	0.083	-0.131
	(0.164)	(0.222)	(0.276)
Constant (θ)	2.411***	1.900***	0.511*
	(0.202)	(0.175)	(0.267)
Observations	672	672	
r^2	0.043	0.048	
F	24.92	23.04	

Pooled OLS; standard errors (wild cluster bootstrap) in parentheses; clustered on village level.
(1) Participants who played Round 1
(2) Participants who played Round 2
Transfers in 1,000 Riel

There is a substantial round effect in the baseline transfer (i.e. θ). Providers with an uninformed recipient without the insurance option send on average 500 Riel less in Round 2 than in Round 1. Yet, there is no significant difference in the transfer sent to informed recipients without the insurance option (i.e. $\theta + \beta$) between Round 1 and Round 2. Most importantly for our analysis, the coefficients of interest, γ and η, do not change significantly across rounds. Thus, a within-subject analysis combining the two rounds is unproblematic.

B.4.3 Determinants of Insurance Uptake

Table B.5: Determinants of Insurance Decision

	(1) Uninformed	(2) Uninformed	(3) Informed	(4) Informed
Age	0.002	0.001	0.001	0.000
	(0.003)	(0.003)	(0.003)	(0.004)
Female	0.069	-0.051	0.033	0.017
	(0.063)	(0.081)	(0.088)	(0.080)
Schooling years	0.002	-0.015	0.001	0.006
	(0.014)	(0.017)	(0.016)	(0.014)
Born in this village	0.137*	0.155*	-0.044	-0.110
	(0.080)	(0.083)	(0.078)	(0.088)
Insurance	-0.259	-0.167	0.209***	0.253**
	(0.168)	(0.137)	(0.069)	(0.103)
Asset Index	0.009	-0.015	-0.581**	-0.498
	(0.316)	(0.329)	(0.234)	(0.293)
Remittances	-0.111	-0.024	0.034	0.096
	(0.068)	(0.068)	(0.077)	(0.086)
Personal Shock	-0.011	-0.015	0.000	0.019
	(0.083)	(0.071)	(0.050)	(0.061)
Agriculture Shock	0.038	0.056	-0.006	0.013
	(0.069)	(0.061)	(0.085)	(0.101)
Trust in Village	0.005	-0.007	0.013	0.020
	(0.025)	(0.023)	(0.020)	(0.026)
Risk Preference	-0.005	-0.006	-0.016	-0.021*
	(0.011)	(0.010)	(0.012)	(0.012)
No. of known participants	0.004	0.012	-0.002	0.005
	(0.010)	(0.013)	(0.010)	(0.010)
No. of related participants	-0.001	-0.003	-0.007*	-0.007*
	(0.005)	(0.007)	(0.003)	(0.004)
Village fixed effects	No	Yes	No	Yes
Observations	168	168	168	168
r_a^2	-0.010	0.130	0.032	0.106
Mean of dependent variable	0.786	0.786	0.714	0.714

Pooled OLS estimator; standard errors in parantheses, clustered on village level.

Appendix C

Appendix for Chapter 4

C.1 Descriptives

C.1.1 Summary Statistics

Table C.1: Socioeconomic Characteristics of Respondents

	mean	sd	min	max	median	count
Individual Characteristics						
Female	0.71	0.45	0	1	1	1270
Age	43.31	13.61	18	79	43	1270
Native	0.67	0.47	0	1	1	1270
Household head	0.47	0.50	0	1	0	1270
School years	2.36	2.97	0	16	1	1270
Working	0.74	0.44	0	1	1	1270
Working outside village	0.14	0.34	0	1	0	936
Selfemployed	0.86	0.35	0	1	1	936
Household Characteristics						
Household size	5.64	2.28	1	18	5	1270
Female headed household	0.28	0.45	0	1	0	1270
Total household income (USD)	164.74	354.69	0	5,000	55	1133
Asset wealth	0.37	0.15	0	1	.37	1270
No electricity	0.71	0.45	0	1	1	1270
Household grows rice	0.91	0.29	0	1	1	1270
Farming as income source	0.77	0.42	0	1	1	1270
Cultivated land (ha)	2.98	3.04	0	50	2.2	1270
Loan at MFI	0.34	0.47	0	1	0	1270
Bank account	0.04	0.19	0	1	0	1270
Savings at fin. institution	0.02	0.15	0	1	0	1270
Insurance	0.08	0.27	0	1	0	1270
Observations	1270					

Table C.2: Variables used in Main Analysis

	mean	sd	min	max	p50	count
$P(T^I < T^{NI})$	0.44	0.50	0	1	0	333
$\frac{T^I - T^{NI}}{T^{NI}}$	-0.23	0.62	-1	5	0	311
Exchange network	6.47	4.44	0	26	6	333
Credit exchange	1.25	1.48	0	8	1	333
Food exchange	3.08	1.58	0	8	3	333
Labor exchange	2.14	2.81	0	19	1	333
Exchange network within family	4.19	3.92	0	21	3	333
Exchange network outside family	2.28	2.79	0	15	1	333
Friendship network	3.63	1.89	0	12	3	333
Advice network	1.17	0.93	0	5	1	333
Workshop network	16.14	9.01	0	31	16	333
Female	0.68	0.47	0	1	1	333
Native	0.71	0.45	0	1	1	333
Age	39.04	12.02	18	77	36	333
Age^2	1,667.75	1,002.28	324	5,929	1,296	333
School years	2.94	3.06	0	14	2	333
Household head	0.46	0.50	0	1	0	333
Asset wealth	0.38	0.15	0	.92	.37	333
Selfemployed	0.73	0.45	0	1	1	333
Cultivated land (ha)	2.95	2.54	0	30	2.5	333
Household grows rice	0.93	0.25	0	1	1	333
Observations	333					

For all provider subjects, excluding replaced participants.

C.1.2 Informal Exchange

Table C.3: Determinants for Network Size

	(1) Friendship	(2) Advice	(3) Credit	(4) Food	(5) Labor(*)	(6) Any exchange
Female	0.016	0.087	0.189**	0.020	0.096	0.081
	(0.04)	(0.06)	(0.08)	(0.03)	(0.09)	(0.05)
Native	0.021	0.151***	0.022	0.111***	0.325***	0.157***
	(0.05)	(0.06)	(0.09)	(0.04)	(0.10)	(0.05)
Age	0.000	0.000	0.016	-0.001	-0.009	0.003
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
Age^2	0.000	0.000	-0.000	0.000	0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
School years	0.014*	0.005	0.020*	0.012**	0.021*	0.016**
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Selfemployed	0.030	-0.018	0.024	0.022	0.096	0.064*
	(0.04)	(0.05)	(0.09)	(0.03)	(0.06)	(0.04)
Cultivated land (ha)	0.012**	0.004	0.025***	0.009	-0.001	0.014**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Female household head	-0.136***	-0.055	-0.301***	-0.121***	-0.119	-0.178***
	(0.03)	(0.06)	(0.09)	(0.03)	(0.08)	(0.05)
Household size	-0.001	-0.026**	-0.007	-0.003	0.003	-0.000
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Observations	1270	1270	1270	1270	1152	1270
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable	3.408	1.159	1.311	2.926	2.171	6.206
r_p^2	0.036	0.021	0.050	0.032	0.171	0.105
χ^2	577.905	256.591	285.991	594.052	580.910	1049.653
p	0.000	0.000	0.000	0.000	0.000	0.000

Estimated by Poisson. Bootstrap standard errors in parentheses. (*) For households growing rice.

Figure depicts for each village the proportion of surveyed households engaged in a specific type of exchange.

Figure C.1: Exchange Networks across Villages

C.2 Regression Analysis

C.2.1 Full Tables

Table C.4: Likelihood of Transfer Reduction and Engagement in Exchange, full table

	(1) $P(T^I < T^{NI})$	(2) $P(T^I < T^{NI})$	(3) $P(T^I < T^{NI})$	(4) $P(T^I < T^{NI})$
Exchange network	0.048*	0.068**	0.066**	0.063*
	(0.03)	(0.03)	(0.03)	(0.03)
Friendship network		-0.087	-0.090	-0.082
		(0.07)	(0.07)	(0.07)
Advice network		0.100	0.103	0.071
		(0.11)	(0.11)	(0.12)
Workshop network		-0.009	-0.011	-0.006
		(0.01)	(0.01)	(0.02)
Female		-0.072	-0.055	0.104
		(0.32)	(0.30)	(0.42)
Native		0.055	0.058	-0.144
		(0.21)	(0.23)	(0.27)
Age		0.088	0.094	0.079
		(0.06)	(0.06)	(0.07)
Age^2		-0.001	-0.001	-0.001
		(0.00)	(0.00)	(0.00)
School years		-0.020	-0.018	0.002
		(0.05)	(0.05)	(0.05)
Household head		0.040	0.027	0.323
		(0.36)	(0.35)	(0.42)
Asset wealth			-0.339	0.359
			(0.91)	(0.99)
Selfemployed			-0.044	0.007
			(0.31)	(0.32)
Cultivated land (ha)			0.027	0.038
			(0.06)	(0.10)
Household grows rice			0.247	0.134
			(0.58)	(0.71)
Observations	333	333	333	333
Village fixed effects	No	No	No	Yes
Mean of dependent variable	0.441	0.441	0.441	0.441
r_p^2	0.008	0.020	0.022	0.022
χ^2	3.405	12.932	20.359	11.855
p	0.065	0.228	0.119	0.618

Estimated by logit. Standard errors in parentheses.

In Columns 1-3: standard errors clustered on village level; in Column 4: bootstrap standard errors.

Constant included but not reported.

Table C.5: Change in Transfers and Engagement in Exchange, full table

	(1) $\frac{T^I-T^{NI}}{T^{NI}}$	(2) $\frac{T^I-T^{NI}}{T^{NI}}$	(3) $\frac{T^I-T^{NI}}{T^{NI}}$	(4) $\frac{T^I-T^{NI}}{T^{NI}}$
Exchange network	-0.023**	-0.037***	-0.035***	-0.030***
	(0.01)	(0.01)	(0.01)	(0.01)
Friendship network		0.054*	0.056*	0.053*
		(0.03)	(0.03)	(0.03)
Advice network		-0.036	-0.046	-0.010
		(0.04)	(0.04)	(0.03)
Workshop network		0.012**	0.012***	0.011**
		(0.00)	(0.00)	(0.00)
Female		0.084	0.090	0.019
		(0.08)	(0.07)	(0.08)
Native		0.070	0.074	0.129**
		(0.07)	(0.07)	(0.05)
Age		-0.024	-0.029*	-0.030*
		(0.02)	(0.02)	(0.02)
Age^2		0.000	0.000*	0.000*
		(0.00)	(0.00)	(0.00)
School years		0.021	0.016	0.011
		(0.01)	(0.01)	(0.02)
Household head		0.059	0.091	0.037
		(0.09)	(0.09)	(0.09)
Asset wealth			0.429	0.203
			(0.32)	(0.21)
Selfemployed			-0.040	-0.058
			(0.08)	(0.09)
Cultivated land (ha)			0.000	-0.002
			(0.01)	(0.01)
Household grows rice			-0.171	0.004
			(0.16)	(0.22)
Observations	311	311	311	311
Village fixed effects	No	No	No	Yes
Mean of dependent variable	-0.232	-0.232	-0.232	-0.232
r_a^2	0.025	0.064	0.066	0.040
F	7.662	2.698	2.420	2.613
p	0.012	0.028	0.035	0.025

Estimated by OLS. Standard errors in parentheses clustered on village level.
Constant included but not reported.

C.2.2 Extended Analysis

Table C.6: Change in Transfers and Credit, Food and Labor Exchange

	(1) $\frac{T^I-T^{NI}}{T^{NI}}$	(2) $\frac{T^I-T^{NI}}{T^{NI}}$	(3) $\frac{T^I-T^{NI}}{T^{NI}}$	(4) $\frac{T^I-T^{NI}}{T^{NI}}$	(5) $\frac{T^I-T^{NI}}{T^{NI}}$	(6) $\frac{T^I-T^{NI}}{T^{NI}}$
Credit exchange	-0.070***			-0.054***	-0.061***	-0.060**
	(0.02)			(0.02)	(0.02)	(0.03)
Food exchange		-0.048**		-0.023	-0.045*	-0.047*
		(0.02)		(0.02)	(0.02)	(0.03)
Labor exchange			-0.023*	-0.010	-0.021*	-0.010
			(0.01)	(0.01)	(0.01)	(0.01)
Friendship network					0.059*	0.058*
					(0.03)	(0.03)
Advice network					-0.039	0.001
					(0.04)	(0.03)
Workshop network					0.012***	0.011**
					(0.00)	(0.00)
Female					0.094	0.027
					(0.07)	(0.08)
Native					0.066	0.123**
					(0.07)	(0.05)
Age					-0.027	-0.028*
					(0.02)	(0.02)
Age^2					0.000	0.000*
					(0.00)	(0.00)
Household head					0.089	0.041
					(0.09)	(0.09)
School years					0.017	0.014
					(0.02)	(0.02)
Asset wealth					0.425	0.187
					(0.31)	(0.21)
Selfemployed					-0.041	-0.060
					(0.08)	(0.08)
Cultivated land (ha)					-0.002	-0.003
					(0.01)	(0.01)
Household grows rice					-0.174	0.012
					(0.16)	(0.21)
Observations	311	311	311	311	311	311
Control Variables	No	No	No	No	Yes	Yes
Village fixed effects	No	No	No	No	No	Yes
r_a^2	0.025	0.012	0.008	0.025	0.066	0.044
F	10.590	5.857	3.068	3.616	2.579	6.074
p	0.004	0.025	0.095	0.031	0.024	0.000

Estimated by OLS. Standard errors in parentheses, clustered on village level.

Control Variables: Friendship network, Advice network, Workshop network, Female, Native, Age, Age^2, Household head, School years, Asset wealth, Selfemployed, Cultivated land (ha), Household grows rice. Constant included but not reported.

Table C.7: Absolute Transfers and Engagement in Exchange

	(1) T^{NI}	(2) T^{NI}	(3) T^{NI}	(4) T^{NI}	(5) T^{I}	(6) T^{I}	(7) T^{I}	(8) T^{I}
Credit exchange	-0.068	-0.057	-0.067	0.000	-0.138***	-0.135***	-0.140***	-0.092
	(0.04)	(0.05)	(0.04)	(0.06)	(0.04)	(0.04)	(0.04)	(0.06)
Food exchange	0.111**	0.140**	0.137**	0.153**	0.061	0.049	0.049	0.043
	(0.04)	(0.06)	(0.06)	(0.07)	(0.04)	(0.06)	(0.05)	(0.06)
Labor exchange	-0.058	-0.055	-0.061	-0.069*	-0.018	-0.035	-0.039	-0.033
	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Friendship network		-0.047	-0.054	-0.058		0.036	0.035	0.021
		(0.06)	(0.06)	(0.06)		(0.06)	(0.06)	(0.06)
Advice network		-0.004	0.033	0.021		-0.056	-0.053	-0.006
		(0.09)	(0.09)	(0.08)		(0.08)	(0.08)	(0.07)
Workshop network		0.015	0.017	0.015		0.032**	0.034***	0.033***
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Female		-0.327*	-0.371*	-0.378*		-0.083	-0.058	-0.195
		(0.18)	(0.21)	(0.21)		(0.15)	(0.15)	(0.17)
Native		-0.156	-0.171	-0.167		-0.221	-0.240	-0.149
		(0.25)	(0.26)	(0.30)		(0.26)	(0.26)	(0.30)
Age		0.048	0.034	0.025		-0.015	-0.037	-0.033
		(0.04)	(0.05)	(0.05)		(0.04)	(0.04)	(0.04)
Age^2		-0.001	-0.001	-0.001		0.000	0.000	0.000
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
School years		0.065**	0.067**	0.033		0.074**	0.064*	0.041
		(0.03)	(0.03)	(0.02)		(0.03)	(0.03)	(0.03)
Household head		0.114	-0.020	0.077		0.026	0.027	-0.055
		(0.24)	(0.22)	(0.25)		(0.24)	(0.24)	(0.24)
Asset wealth			-0.307	0.756			0.856	1.150*
			(0.87)	(0.68)			(0.56)	(0.62)
Selfemployed			0.527**	0.561**			0.262	0.198
			(0.21)	(0.25)			(0.23)	(0.19)
Cultivated land (ha)			-0.016	-0.035			-0.017	-0.046
			(0.04)	(0.04)			(0.03)	(0.03)
Household grows rice			-0.331	-0.675			-0.234	-0.289
			(0.57)	(0.71)			(0.37)	(0.46)
Observations	333	333	333	333	333	333	333	333
Village fixed effects	No	No	No	Yes	No	No	No	Yes
Mean of dep. variable	2.150	2.150	2.150	2.150	1.556	1.556	1.556	1.556
r_a^2	0.006	0.019	0.022	0.031	0.009	0.052	0.053	0.034
F	4.038	6.707	19.261	8.482	5.552	6.380	25.536	21.825
p	0.021	0.000	0.000	0.000	0.006	0.000	0.000	0.000

Estimated by OLS. Standard errors in parentheses clustered on village level. Constant included but not reported.

Table C.8: Change in Transfers and Engagement in Exchange, excluding Outliers(*)

	(1) $P(T^I < T^{NI})$	(2) $P(T^I < T^{NI})$	(3) $P(T^I < T^{NI})$	(4) $\frac{T^I - T^{NI}}{T^{NI}}$	(5) $\frac{T^I - T^{NI}}{T^{NI}}$	(6) $\frac{T^I - T^{NI}}{T^{NI}}$
Exchange network	0.035	0.047	0.040	-0.024**	-0.036***	-0.033**
	(0.03)	(0.03)	(0.04)	(0.01)	(0.01)	(0.01)
Friendship network		-0.074	-0.073		0.058*	0.058*
		(0.07)	(0.07)		(0.03)	(0.03)
Advice network		0.165	0.137		-0.057	-0.020
		(0.13)	(0.14)		(0.05)	(0.04)
Workshop network		-0.010	-0.005		0.012**	0.011**
		(0.01)	(0.02)		(0.00)	(0.00)
Female		-0.053	0.047		0.084	0.018
		(0.30)	(0.31)		(0.07)	(0.08)
Native		0.007	-0.151		0.076	0.127**
		(0.21)	(0.29)		(0.07)	(0.05)
Age		0.098	0.090		-0.030*	-0.032*
		(0.06)	(0.08)		(0.02)	(0.02)
Age^2		-0.001	-0.001		0.000*	0.000*
		(0.00)	(0.00)		(0.00)	(0.00)
School years		-0.016	0.006		0.014	0.010
		(0.05)	(0.06)		(0.02)	(0.02)
Household head		0.086	0.361		0.078	0.024
		(0.33)	(0.38)		(0.10)	(0.09)
Asset wealth		-0.121	0.744		0.406	0.180
		(0.88)	(0.92)		(0.32)	(0.21)
Selfemployed		-0.053	-0.019		-0.041	-0.060
		(0.32)	(0.37)		(0.09)	(0.09)
Cultivated land (ha)		0.013	0.012		0.001	-0.003
		(0.05)	(0.11)		(0.01)	(0.01)
Household grows rice		0.307	0.221		-0.171	0.014
		(0.58)	(0.77)		(0.16)	(0.22)
Observations	319	319	319	298	298	298
Village fixed effects	No	No	Yes	No	No	Yes
Mean of dep. variable	0.433	0.433	0.433	-0.219	-0.219	-0.219
r_p^2	0.003	0.018	0.022			
r_a^2				0.016	0.055	0.037
χ^2	1.457	22.751	33.145			
F				5.533	2.393	2.028
p	0.227	0.064	0.003	0.029	0.037	0.072

(*)Subjects reporting 16 or more exchange arrangements are excluded.

Columns 1-3 estimated by logit; columns 4-6 estimated by OLS.

Standard errors clustered on village level.

Column 6 bootstrap standard errors. Constant included but not reported.

Erklärung

Ich versichere, dass ich die von mir vorgelegte Dissertation selbstständig und ohne unerlaubte Hilfe angefertigt habe und mich keiner anderen als der in ihr angegebenen Hilfsmittel bedient habe. Sämtliche Zitate aus anderen Quellen sind als solche gekennzeichnet und mit Quellenangaben versehen. Zudem versichere ich, dass ich mich noch nie zuvor einem Doktorexamen unterzogen habe, und dass die vorgelegte Dissertation keiner anderen Fakultät oder einer ihrer Mitglieder vorgelegen hat.

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Berlin, 14.03.2017,